PHILOSOPHICAL TRANSACTIONS,

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ROYAL SOCIETY

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PART I.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume: the Society, as a Body, never intereſting themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſable, that a Committee of their members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

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It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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THE President and Council of the Royal Society adjudged, for the Year 1787, the Medal on Sir Godfrey Copley's Donation, to John Hunter, Eſq. F. R. S. for three Papers which were printed in the laſt Volume of the Philoſophical Tranſactions.

PHILOSOPHICAL

TRANSACTIONS.

I. *Of the Methods of manifeſting the Preſence, and aſcertaining the Quality, of ſmall Quantities of Natural or Artificial Electricity. By Mr.* Tiberius Cavallo, *F. R. S.*

The Lecture founded by the late Henry Baker, Eſq. F. R. S.

Read November 15, 1787.

THE perſevering induſtry of many ingenious perſons in the philoſophical world, ſince the beginning of the preſent century, has gradually, and with rapid ſucceſs, examined the effects, aſcertained the properties, and inveſtigated the laws of a ſcience almoſt entirely unknown before. Various wonderful phænomena of nature have been explained, means have been diſcovered of avoiding ſome of their dangerous effects, and a greater inſight into the general operations of nature has been derived, from the knowledge of ſo great and ſo extenſive a power as is commonly underſtood under the name of *electricity*; yet, if inſtead of contemplating its more ſtriking phænomena, *viz.* thoſe which are apt to ſurpriſe more by the magnitude of their effects, than by the inſtruction which they may afford, we endeavour to examine the ſubject more cloſely, we ſhall find that our knowledge of electricity goes very little, if at all, beyond the ſuperficial part of it. We know, for inſtance, that a piece of glaſs, or other electric, when rubbed, will produce that power which we call electricity, that the glaſs will communicate the acquired electricity to a piece of metal, that the piece of metal will retain that power in certain circumſtances, and ſo on; but no perſon has ſhewn how that power is generated by the friction, or what prevents its paſſage through the ſubſtance of ſome particular bodies. It has been aſcertained, that the air of moſt countries, and probably of the whole world, as well as the clouds, fogs, rains, &c. are almoſt always electrified; but we are ignorant of the office which this electricity can have in the great laboratory of nature; for ſurely ſo general and ſo active a power can hardly be intended by nature, merely to intimidate mankind now and then with the thunder and the lightning.

It appears, therefore, that thoſe perſons, who are now willing to diſtinguiſh themſelves in this intereſting branch of natural philoſophy, ought to examine the electrical power not ſo much in its accumulated, as in its incipient ſtate. Its firſt origin, or very beginning, ought to be inveſtigated, it being afterwards very eaſy to underſtand its increaſe; for we may eaſily comprehend, how a great quantity of it may be accumulated from the often repeated additions of its ſmalleſt portions.

This truth having been underſtood by ſeveral philoſophical perſons, has induced them to contrive inſtruments neceſſary for the purpoſe, and to make numerous obſervations, which have undoubtedly promoted the knowledge of the ſubject; but a great deal ſtill remains to be done, before we can attain to the knowledge of the object in view, *viz.* of the real nature, of the firſt origin, and of the general uſe of electricity. The inſtruments hitherto invented are ſtill inadequate to the purpoſe, and the known methods of operating are not free from conſiderable objections. To examine the peculiar conſtructions, intended uſes, properties, and defects, of thoſe inſtruments, as well as methods of performing the experiments, is the principal object of the preſent Lecture, which I have the honour of delivering to this learned Society, and which may, perhaps, be of uſe to other experimenters in this branch of philoſophy; ſince what is mentioned in the following pages is the reſult of long experience, and of conſideration. Of experience, which has often contradicted the ſanguine expectations of a pre-conceived theory; and of conſideration, which has frequently demonſtrated the abſurdity or inſufficiency of ſchemes which, at firſt ſight, ſeemed very eaſy and plauſible.

The late Mr. JOHN CANTON was, as far as I know, the firſt perſon who conſtructed an electrometer, or inſtrument capable of ſhewing the preſence of what was then conſidered as a ſmall quantity of electricity. This inſtrument conſiſted of two ſmall balls of pith of elder or of cork, faſtened to the two extremities of a linen thread, the middle of which was faſtened to an oblong wooden box, in which the thread and balls were kept, when not actually in uſe. It was with ſuch an inſtrument that Mr. CANTON himſelf, Father BECCARIA, and others, aſcertained the electricity generally exiſting in the air; and that Mr. RONAYNE diſcovered the conſtant electricity of fogs. But in the courſe of my experiments, having made frequent uſe of ſuch electrometers, it naturally occurred to me, that in ſeveral caſes, when the electrometer gave no ſigns of electricity, or at leaſt not ſufficient to aſcertain its quality, the cauſe of it was the reſpectively large ſize of the inſtrument; for a ſmall quantity of electricity being diffuſed through the box, thread, and balls of the electrometer, had not power ſufficient to ſeparate the balls, and of courſe to ſhew its preſence. In conſequence of this, I contracted the ſize of the electrometers to ſuch a degree as could be affected by leſs than the tenth part of that quantity of electricity which was neceſſary to affect Mr. CANTON's electrometer. But in making the electrometers very ſhort, the ſtiffneſs of the threads, which had been inſignificant in a great extent, became now very conſiderable; hence, inſtead of faſtening the balls to the two extremities of one piece of thread, I found it neceſſary to ſuſpend each ball by a ſeparate piece of thread, the upper part of which was formed into a loop, which moved in a ring of braſs wire.

The electrometers, thus improved, were ſtill ſubject to a very great imperfection, which was the twiſting of the threads; to avoid which I ſubſtituted fine ſilver wire, inſtead of linen threads, which anſwered very well. However, in obſerving the electricity of the atmoſphere, thoſe electrometers appeared to labour under a conſiderable inconvenience, which was their being diſturbed by the wind; for it frequently happened, that neither the exiſtence, nor the quality, of the electricity could be aſcertained on account of the wind agitating the inſtrument, and becauſe the electricity being in ſmall quantity would not remain on the electrometer a time ſufficient to bring the inſtrument within doors, where it might be examined without any obſtruction. In order to remove this imperfection, I incloſed the electrometer in a bottle, as it is deſcribed in the Philoſophical Tranſactions, Vol. LXX. which conſtruction has been found to anſwer remarkably well.

This bottle electrometer has been ſince altered by various perſons; though, in my opinion, thoſe alterations do not tend to improve it altogether. M. DE SAUSSURE, by altering the ſhape of the bottle, and depriving it of a neck, has rendered it capable of retaining the communicated electricity only for a very ſhort time*; whereas, ſome of thoſe electrometers, conſtructed on my original plan, have retained the communicated electricity for more than four hours.

Beſides this, M. DE SAUSSURE, as well as ſome who make theſe electrometers for ſale, have ſubſtituted pith balls, inſtead of the conical corks of the original plan, the latter of which are preferable for two reaſons; *viz.* firſt, becauſe the balls of pith are apt to adhere to each other, ſo that on communicating the electricity they often do not ſhew any repulſion at all; or if the communicated electricity be conſiderably great, the pith balls, after adhering to each other for ſome time, are at laſt ſeparated with violence, ſo as to fly to the ſides of the bottle, which fruſtrates the experiment; whereas the corks are not near ſo ſubject to this inconvenience; and, ſecondly, becauſe the conical ſhape renders thoſe corks capable of preſenting a much greater ſurface to each other, than if they were globular, their weights being the ſame in both caſes; for it is very well known, that, of all the ſolid figures, the ſphere contains the greateſt capacity under the leaſt ſurface; and as the effects of electricity, in thoſe caſes, is proportionate to the ſurface, and not to the quantity of matter, it follows, that the conical ſhape is preferable to the globular.

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* SAUSSURE's Voyage dans les Alpes, Tom. II.

Another alteration of the bottle electrometer was lately made by the Rev. Mr. BENNET, and is deſcribed in the Philoſophical Tranſactions, Vol. LXXVII. Part I. It conſiſts principally in ſubſtituting two ſlips of gold-leaf to the corks ſuſpended by wires. This alteration has ſome peculiar advantages and diſadvantages. Its advantages are in general a greater degree of ſenſibility, and a more eaſy conſtruction. Its diſadvantages are, firſt, that the inſtrument is not portable; and, ſecondly, that even when not carried about, it is apt to be ſpoiled very eaſily. However, in ſome caſes it is very uſeful, ſo that, upon the whole, it may be conſidered as a very good improvement.

As the faſtening of the ſlips of gold-leaf to this electrometer, and to let them hang parallel, is rather difficult, it will not be improper to deſcribe a method which I have contrived, in order to accompliſh it more eaſily. When the ſlips are cut, and are lying upon paper or leather, I make them equal in length, by meaſuring them with a pair of compaſſes, and cutting off a portion from the longeſt: then cut two bits of very fine paper, each about half an inch long, and a quarter of an inch broad, and ſtick one of them, by means of a little wax, which is rubbed upon them, to one extremity of each ſlip of gold-leaf, ſo as to form a kind of letter T. This done, I hold up, in the fingers of one hand, one piece of paper, with the gold leaf ſuſpended to it, and hold the other with the other hand;

then bring them near to each other, and after having adjuſted them properly, ſo that the ſlips may hang parallel and ſmooth, I force the pieces of paper, which now touch each other, between the two ſides of a ſort of pincers made of braſs wire, which are faſtened to the under part of that piece which forms the top of the electrometer: and, in order to change thoſe ſlips when ſpoiled, I keep in a book ſeveral ſlips of gold-leaf ready cut, and furniſhed each with a piece of paper; ſo that by this means this electrometer is rendered, in a certain manner, portable.

Beſides the way of aſcertaining ſmall quantities of electricity, by means of very delicate electrometers, two methods have been communicated to the philoſophical world, by which ſuch quantities of electricity may be rendered manifeſt, as could not be perceived by other means. The firſt of thoſe methods is an invention of M. VOLTA, the apparatus for it being called the *Condenſer of Electricity*, and is deſcribed in the Philoſophical Tranſactions, Vol. LXXII. The ſecond is a contrivance of the above-mentioned Mr. BENNET, who calls the apparatus *The Doubler of Electricity.* A deſcription of it is inſerted in the Philoſophical Tranſactions, Vol. LXXVII.

M. VOLTA's condenſer conſiſts of a flat and ſmooth metal plate, furniſhed with an inſulating handle, and a ſemi-conducting, or imperfectly inſulating, plane. When one wiſhes to examine a weak electricity with this apparatus, as that of the air in calm and hot weather, which is not generally ſenſible to an electrometer, he muſt place the above-mentioned plate upon the ſemi-conducting plane, and a wire, or ſome other conducting ſubſtance, muſt be connected with the metal plate, and muſt be extended in the open air, ſo as to abſorb its electricity; then, after a certain time, the metal plate muſt be ſeparated from the ſemi-conducting plane, and being preſented to an electrometer will electrify it much more than if it had not been placed upon the above-mentioned plane.

The principle on which the action of this apparatus depends is, that the metal plate, whilſt ſtanding contiguous to the ſemi-conducting plane, will both abſorb and retain a much greater quantity of electricity than it can either abſorb or retain when ſeparate, its capacity being increaſed in the former, and diminiſhed in the latter caſe.

Whoever conſiders this apparatus, will eaſily find, that its office is not to manifeſt a ſmall quantity of electricity, but to condenſe an expanded quantity of electricity into a ſmall ſpace; hence, if by means of this apparatus one expected to render more manifeſt than it generally is, when communicated immediately to an electrometer, the electricity of a ſmall tourmalin, or of a hair when rubbed, he would find himſelf miſtaken.

It is Mr. BENNET's doubler that was intended to anſwer that end; *viz.* to multiply, by repeated doubling, a ſmall, and otherwiſe unperceivable, quantity of electricity, till it became ſufficient to affect an electrometer, to give ſparks, &c. The merit of this invention is certainly conſiderable; but the uſe of it is far from preciſe and certain.

This apparatus conſiſts of three braſs plates, which we ſhall call A, B, and C; each of which is about three or four inches in diameter. The firſt plate A is placed upon the gold-leaf electrometer, or it may be ſupported horizontally by any other inſulating ſtand, and its upper part only is thinly varniſhed. The ſecond plate B is varniſhed on both ſides, and is furniſhed with an inſulating handle, which is faſtened laterally to the edge of it. The third plate C is varniſhed on the under ſide only, and is furniſhed with an inſulating handle, which is perpendicular to its upper ſurface.

This apparatus is uſed in the following manner. The plate B being laid upon the plate A, the ſmall quantity of electricity, which is required to be multiplied, is communicated to the under part of the plate A, and at the ſame time the upper part of B is touched with a finger; then the finger is firſt removed; the plate B is afterwards removed from over the plate A. The plate C is now laid upon B, and its upper ſurface is touched, for a ſhort time, with a finger. By this operation it is clear, that if the electricity communicated to the plate A is poſitive, the plate B muſt have acquired a negative electricity, and the plate C muſt have acquired the poſitive, *viz.* the ſame of the plate A. Now the plate B, being ſeparated from C, is laid as before upon A; the edge of C is brought into contact with the under part of the plate A, and at the ſame time the upper part of B is touched with a finger, by which means the plate B, being acted upon by the atmoſpheres of both the plates A and C, will acquire nearly twice as much electricity as it did the firſt time, and of courſe will render the plate C, when that is laid upon it, proportionably more electrified than before: thus, by repeating this operation, the electricity may be increaſed to any required degree.

The varniſh on thoſe ſurfaces of the plates which are to lie contiguous to each other ſerves to prevent the metal of one touching the metal of the other; for in that caſe, inſtead of one plate cauſing a contrary electricity in the other, the electricity of the firſt would be gradually communicated to the others, and would be diſſipated.

As ſoon as I underſtood the principle of this contrivance, I haſtened to conſtruct ſuch an apparatus, in order to try ſeveral experiments of a very delicate nature, eſpecially on animal bodies and vegetables, which could not have been attempted before, for want of a method of aſcertaining exceedingly ſmall quantities of electricity; but, after a great deal of trouble, and many experiments, I was at laſt forced to conclude, that the doubler of electricity is not an inſtrument to be depended upon, for this principal reaſon; *viz.* becauſe it multiplies not only the electricity which is willingly communicated to it from the ſubſtance in queſtion; but it multiplies alſo that electricity which in the courſe of the operation is almoſt unavoidably produced by accidental friction; or that quantity of electricity, however ſmall it may be, which adheres to the plates in ſpite of every care and precaution.

Having found, that with a doubler conſtructed in the above deſcribed manner, after doubling or multiplying twenty or thirty times, it always became ſtrongly electrified, though no electricity had been communicated to it before the operation, and though every endeavour of depriving it of any adhering electricity had been practiſed; I naturally attributed that electricity which appeared after repeatedly doubling, to ſome friction given to the varniſh of the plates in the courſe of the operation. In order to avoid entirely this ſource of miſtake, or at leaſt of ſuſpicion, I conſtructed three plates without the leaſt varniſh, and which, of courſe, could not touch each other, but were to ſtand only within about one-eighth of an inch of each other. To effect this, each plate ſtood vertical, and was ſupported by two glaſs ſticks, which were covered with ſealing-wax: ſee Tab. I. fig. 1. and 2., where AB is a wooden pedeſtal, 7½ inches long, 2½ broad, and 1¼ inch thick; C and D are the two glaſs ſticks cemented into the ſtand or pedeſtal AB, and likewiſe into the piece of wood E, which is faſtened to the back of the plate. The plate itſelf is of ſtrong tin, and meaſures about eight inches in diameter. The ſtand AB projects very little before the plate, by which means, when two of thoſe plates are placed upon a table facing each other, the wooden ſtands will prevent their coming into actual contact, as may be clearly perceived in fig. 3.

I need not deſcribe the manner of doubling or of multiplying with thoſe plates; the operation being eſſentially the ſame as when the plates are conſtructed according to Mr. BENNET's original plan, excepting that, inſtead of placing them one upon the other, mine are placed facing each other; and in performing the operation they are laid hold of by the wooden ſtand AB; ſo that no friction can take place either upon the glaſs legs, or upon any varniſh; for theſe plates have no need of being varniſhed. Sometimes, inſtead of touching the plates themſelves with the finger, I have fixed a piece of thin wire to the back of the plate, and have then applied the finger to the extremity of the wire, ſuſpecting that ſome friction and ſome electricity might poſſibly be produced when the finger was applied in full contact to the plate itſelf.

It is evident, that as the plates do not come ſo near to each other in this, as they do in the other conſtruction, the electricity of one of them cannot produce ſo great a quantity of the contrary electricity in the oppoſite plate; hence, in this conſtruction, it will be neceſſary to continue the operation of doubling ſomewhat longer; but this diſadvantage is more than repaid by the certainty of avoiding any friction.

Having conſtructed thoſe plates, I thought that I might proceed to perform the intended experiments without any further obſtruction; but in this I found myſelf quite miſtaken: for, on trying to multiply with thoſe new plates, and when no electricity had been previouſly communicated to any of them, I found, that after doubling ten, fifteen, or at moſt twenty times, they became ſo full of electricity as to afford even ſparks. All my endeavours to deprive them of electricity proved ineffectual. Neither expoſing them, and eſpecially the glaſs ſticks, to the flame of burning paper, nor breathing upon them repeatedly, nor leaving them untouched for ſeveral days, and even for a whole month, during which time the plates remained connected with the ground by means of good conductors, nor any other precaution I could think of, was found capable of depriving them of every veſtige of electricity; ſo that they might ſhew none after doubling ten, fifteen, or at moſt twenty times.

The electricity produced by them was not always of the ſame ſort; for ſometimes it was negative for two or three days together; at other times it was poſitive for two or three days more; and often it changed in every operation. This made me ſuſpect, that poſſibly the beginning of that electricity was derived from my body, and being communicated by the finger to the plate that was firſt touched, was afterwards multiplied. In order to clear this ſuſpicion, I actually tried thoſe plates at different times, *viz.* before and after having walked a great deal, before and after dinner, &c. noting very accurately the quality of the electricity produced each time; but the effects ſeemed to be quite unconnected with the above-mentioned concomitant circumſtances, which independence was further confirmed by obſerving that the electricity produced by the plates was of a fluctuating nature, even when, inſtead of touching the plates with the finger, they had been touched with a wire, which was connected with the ground, and which I managed by means of an inſulating handle.

At laſt, after a great variety of experiments, which it is unneceſſary to deſcribe, I became fully convinced, that thoſe plates did always retain a ſmall quantity of electricity, perhaps of that ſort with which they had been laſt electrified, and of which it was almoſt impoſſible to deprive them. The various quality of the electricity produced was owing to this, *viz.* that as one of thoſe plates was poſſeſſed of a ſmall quantity of poſitive electricity, and another was poſſeſſed of the negative electricity, that plate which happened to be the moſt powerful, occaſioned a contrary electricity in the other plate, and finally produced an accumulation of that particular ſort of electricity.

Thoſe obſervations evidently ſhew, that no preciſe reſult can be obtained from the uſe of thoſe plates, and of courſe that, when conſtructed according to the original plan, they are ſtill more equivocal, becauſe they admit of more ſources of miſtake.

As thoſe plates, after doubling or multiplying only four or five times, ſhew no ſigns of electricity, none having been communicated to them before, I imagined that they might be uſeful ſo far only, *viz.* that when a ſmall quantity of electricity is communicated to any of them in the courſe of ſome experiment, one might multiply it with ſafety four or five times, which would even be of advantage in various caſes; but in this alſo my expectations were diſappointed, as will appear from the following pages.

Having obſerved, after many experiments, that, *ceteris paribus*, when I began to multiply from a certain plate, for inſtance A, the electricity which reſulted was generally poſitive; and when I began with another plate B, *viz.* conſidered this plate B as the firſt plate, the reſulting electricity was generally negative; I communicated ſome negative electricity to the plate A, with a view of deſtroying its inherent poſitive electricity. This plate A being now electrified negatively, but ſo weakly as juſt to affect an electrometer, I began doubling; but after having doubled three or four times, I found, by the help of an electrometer, that the communicated negative electricity in the plate was diminiſhed inſtead of being increaſed; ſo that ſometimes it vaniſhed entirely, though by continuing the operation it often began to increaſe again, after a certain period. This ſhews, that the quantity of electricity, which however ſmall it may be, remains in a manner faſtened to the plates, will help either to increaſe or to diminiſh the accumulation or multiplication of the communicated electricity, according as it happens to be of the ſame, or of a different nature.

After all the above-mentioned experiments made with thoſe doubling or multiplying plates, we may come to the following concluſion, *viz.* that the invention is very ingenious, but their uſe is by no means to be depended upon. It is to be wiſhed, that they may be improved, ſo as to obviate the weighty objections that have been mentioned in the preceding pages, the firſt deſideratum being to conſtruct a ſet of ſuch plates as, when no electricity is communicated, they will produce none after having performed the operation of doubling for a certain number of times.

Upon the whole, the methods by which ſmall quantities of electricity may be aſcertained with preciſion are, as far as I know, only three. If the abſolute quantity of electricity be ſmall and pretty well condenſed, as that produced by a ſmall tourmalin when heated, or by a hair when rubbed, the only effectual method of manifeſting its preſence, and aſcertaining its quality, is to communicate it immediately to a very delicate electrometer, *viz.* a very light one, that has no great extent of metallic or of other conducting ſubſtance; becauſe if the ſmall quantity of electricity that is communicated to it be expanded throughout a proportionably great ſurface, its elaſticity, and of courſe its power of ſeparating the corks of an electrometer, will be diminiſhed in the ſame proportion.

The other caſe is, when one wants to aſcertain the preſence of a conſiderable quantity of electricity, which is diſperſed or expanded into a great ſpace, and is little condenſed, like the conſtant electricity of the atmoſphere in clear weather, or like the electricity which remains in a large Leyden phial after the firſt or ſecond diſcharge.

To effect this, I uſe an apparatus, which in principle is nothing more than Mr. VOLTA's condenſer; but with certain alterations, which render it leſs efficacious than in the original plan, but at the ſame time render it much leſs ſubject to equivocal reſults. I place two of the above deſcribed tin plates upon a table, facing each other, as ſhewn in fig. 3. and about one-eighth of an inch aſunder. One of thoſe plates, for inſtance A, is connected with the floor by means of a wire, and the other plate B is made to communicate, by any convenient means, with the electricity that is required to be collected. In this diſpoſition the plate B, on account of the proximity of the other plate, will imbibe more electricity than if it ſtood far from it, the plate A in this caſe acting like the ſemi-conducting plane of M. VOLTA's condenſer, though not with quite an equal effect, becauſe the other plate B does not touch it; but yet, for the very ſame reaſon, this method is incomparably leſs ſubject to any equivocal reſult. When the plates have remained in the ſaid ſituation for the time that may be judged neceſſary, the communication between the plate B, and the

conducting

conducting ſubſtance which conveyed the electricity to it, muſt be diſcontinued by means of a glaſs ſtick, or other inſulating body; then the plate A is removed, and the plate B is preſented to an electrometer, in order to aſcertain the quality of the electricity; but if the electrometer be not affected by it, then the plate B is brought with its edge into contact with another very ſmall plate, which ſtands upon a ſemi-conducting plane, after the manner of M. Volta's condenſer *; which done, the ſmall plate, being held by its inſulating handle, is removed from the inferior plane, and is preſented to the electrometer: and it frequently happens, that the ſmall plate will affect the electrometer very ſenſibly, and quite ſufficient for the purpoſe; whereas the large plate itſelf ſhewed no clear ſigns of electricity.

If it be aſked, why I uſe the ſemi-conducting plane for this ſmall plate, and not for the large one? the anſwer is, firſt, becauſe the large ſemi-conducting plane is incomparably more difficult to be procured than the ſmall one; and, ſecondly, becauſe the ſmall plane may be eaſily deprived of any accidental electricity which may adhere to it; but the large one is more difficultly rendered fit for the purpoſe, eſpecially as the large plate ought in general to remain upon it a much longer time than the ſmall plate is to remain upon its ſemi-conducting plane.

The third and laſt caſe is when the electricity to be aſcertained is neither very conſiderable in quantity, nor much condenſed; ſuch is the electricity of the hair of certain animals, of the ſurface of chocolate when cooling, &c. In this caſe the beſt method is to apply a metal plate, furniſhed with an

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* This ſmall plate is nearly of the ſize of a ſhilling, and the ſemi-conducting plane is of wood covered with copal varniſh.

inſulating handle, like an electrophorus plate, to the electrified body, and to touch this plate with a finger for a ſhort time whilſt ſtanding in that ſituation; which done, the plate is removed, and is brought near an electrometer; or its electricity may be communicated to the plate of a ſmall condenſer, as directed in the preceding caſe, which will render the electricity more conſpicuous. It is evident, that in this caſe the metal plate will acquire the electricity contrary to that of the ſubſtance in queſtion; but this anſwers the ſame purpoſe: for if the electricity of the plate be found to be poſitive, one muſt conclude, that the electricity of the body in queſtion is negative, and contrariwiſe. In this operation, care muſt be had not to put the metal plate too near, or in full contact with the ſubſtance to be examined, leſt the friction, likely to happen between the plate and the body, ſhould produce ſome electricity, the origin of which might be attributed to other cauſes.

Having thus far deſcribed the ſureſt methods of aſcertaining the preſence and quality of electricity, when its quantity or degree of condenſation is ſmall, I ſhall now beg leave to add ſome farther remarks on the ſubject of electricity in general, and which have been principally ſuggeſted by what has been mentioned in the preceding pages.

On the hypotheſis of a ſingle electric fluid, it is ſaid, that every ſubſtance in nature, when not electrified, contains its proper ſhare of electric fluid, which is proportionate to its bulk, or to ſome other of its properties; and it is generally believed, that this equal or proportionate diſtribution of electric fluid takes place with the greateſt part of natural bodies. However, the fact is far from being ſo; and I may venture to aſſert that, ſtrictly ſpeaking, every ſubſtance is always electrified, *viz.* that every ſubſtance, and even the various parts of the ſame body, contain at all times more or leſs electric fluid than that quantity of it which it ought to contain, in order to be in an electrical equilibrium with the bodies that ſurround it.

At firſt ſight it may be thought quite immaterial to know, whether the electric fluid is diſperſed in the juſt proportion among the various ſubſtances which are not looked upon as electrified, or whether it deviates in a ſmall degree from that proportionate diſtribution; but it will hereafter appear, that one of thoſe aſſertions will lead us to the explanation of an intereſting phænomenon in electricity, whereas the other does not admit of it; beſides, what is called ſmall difference of the proportionate diſtribution, inſomuch as it does not affect our inſtruments, may be ſufficient for ſeveral operations of nature, which it is our intereſt to inveſtigate.

If we inquire what phænomena evince this altered diſtribution, or the actually electrified ſtate of all bodies, the preceding obſervations will furniſh ſome very unequivocal ones; eſpecially that of the doubling plates made after my plan, which ſhewed to be electrified even after having remained untouched for a whole month, during which time they had been in communication with the ground; for if each of them had contained an equal ſhare of electric fluid, the electric atmoſphere of one of them could not poſſibly occaſion a contrary electricity in the other, and conſequently no accumulation of that power could have happened.

A great number of inſtances are related in books on the ſubject of electricity, and in the Philoſophical Tranſactions, of pieces of glaſs, of ſulphur, of ſealing-wax, &c. having remained electrified ſo far as to affect an electrometer for months after they had been excited, or even touched; but the following experiment will ſhew, in a clearer manner, the great length of time that a quantity of electricity will remain upon a body.

Having conſtructed a gold-leaf electrometer in the niceſt manner I could, and which, on account of the non-conducting nature and conſtruction of its upper part, could remain ſenſibly electrified for ſeveral hours together, I communicated ſome electricity to it, which cauſed the ſlips of gold-leaf to diverge with a certain angle; and as the electricity was gradually diſſipated, the divergency diminiſhed in the ſame proportion. Now, whilſt this diminution of divergency was going on, I looked through a ſmall teleſcope, and by means of a micrometer meaſured the chords of the angles of divergency, ſetting down the time elapſed between each pair of contiguous obſervations; and as the chord of the angle of divarication is in the direct ſimple proportion of the denſity of the electric fluid *, I could by this means know how much electric fluid was loſt by the electrometer in a certain time, and of courſe what portion of the electricity firſt communicated to the electrometer ſtill remained in it. Let us make the chord of the angle of divarication on firſt electrifying the electrometer, or rather when firſt obſerved, equal to 16; or let us conceive that quantity of electricity to be diviſible into 16 equal parts.

I obſerved, that when the chord of the angle became equal to eight, the time elapſed between this and the firſt obſervation was one minute; when the chord became equal to four, the time elapſed between this point and the preceeding obſervation was 3′ 30″; when the chord became equal to two, the time elapſed ſince the preceding obſervation was 17′; and when the chord became equal to one, the time elapſed ſince the

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* This propoſition was firſt aſcertained by F. Beccaria. See Philoſphical Tranſactions, Vol. LVI.

preceding obſervation was one hour and a quarter; after which the electrometer remained ſenſibly electrified for a long time.

In repeating this experiment, the times elapſed between the correſponding obſervations did not follow ſtrictly the ſame proportion of increaſe; nor did they increaſe regularly in the ſame experiments, which may be attributed in great meaſure to the inaccuracy in obſerving, and to the fluctuating ſtate of the air; but it could be ſafely inferred from all the experiments, that the times required for the diſperſion of the electricity were at leaſt greater than the inverſe duplicate proportion of the denſities of the electricity remaining in the electrometer. And if we imagine, that they continue to diminiſh in the ſame proportion of increaſing time, which is far from being an extravagant ſuppoſition, we ſhall find, by a very eaſy calculation, that about two years after the electrometer would ſtill retain the hundredth part of the electricity communicated to it in the beginning of the experiment; and as we do not know how far a quantity of electricity is diviſible, or to what extent it may be expanded, we may conclude with ſaying that, ſtrictly ſpeaking, the electrometer would remain electrified for many years.

It may be inferred from this, as well as from many other experiments, that the air, or in general any ſubſtance, is a more or leſs perfect conductor of electricity, according as the electricity which is to paſs through it is more or leſs condenſed; ſo that if a given quantity of electric fluid be communicated to a ſmall braſs ball, one may take it away by ſimply touching the ball with a finger; but if the ſame quantity of electric fluid be communicated to a ſurface of about 100 or 1000 ſquare feet, the touching with the finger will hardly take away any part of it.

If it be aſked, what power communicates the electricity, or originally diſturbs the equilibrium of the natural quantity of electric fluid in the various bodies of the univerſe; we may anſwer, that the fluctuating electric ſtate of the air, the paſſage of electrified clouds, the evaporation and condenſation of fluids, and the friction ariſing from divers cauſes, are perpetually acting upon the electric fluid of all bodies, ſo as either to increaſe or diminiſh it, and that to a more conſiderable degree than is generally imagined.

I ſhall laſtly conclude, with briefly propoſing an explanation of the production of electricity by friction, which is dependent upon the above ſtated propoſition, *viz.* that bodies are always electrified in ſome degree; and likewiſe upon the well known principle of the capacity of bodies for holding electric fluid being increaſed by the proximity of other bodies in certain circumſtances.

It ſeems to me, that the cylinder of an electrical machine, like ABC in fig. 4. muſt always retain ſome electricity of the poſitive kind, though not equally denſe in every part of its ſurface; therefore, when the part of it A is ſet contiguous to the rubber *fg*, it muſt induce a negative electricity in the rubber. Now, when by turning the cylinder, another part of it B (which ſuppoſe to have a leſs quantity of poſitive electricity than the preceding part A) comes quickly againſt the rubber; the rubber being already negative, and not being capable of loſing that electricity very quickly, muſt induce a ſtronger poſitive electricity in the part B, which is now oppoſite to it; but this part B cannot become more poſitively electrified, unleſs it receives the electric fluid from ſome other body, and therefore ſome quantity of electric fluid paſſes from the loweſt part of the rubber to the part B of the glaſs, which additional quantity of electric fluid is retained by the part B only whilſt it remains in contact with the rubber; for after that, its capacity being diminiſhed, the electric fluid endeavours to eſcape from it. Thus we may conceive how every other part of the glaſs acquires the electric fluid, &c. and what is ſaid of the cylinder of an electrical machine may, with proper changes, be applied to any other electric and its rubber.

It appears, therefore, that according to this theory, a part of the rubber, *viz.* that which the ſurface of the glaſs cylinder enters in turning round, muſt ſerve to furniſh the electric fluid to the glaſs, and the upper part muſt be poſſeſſed of a negative electricity capable of inducing a poſitive electricity in the glaſs contiguous to it. In fact, this ſeems to be confirmed by the general practice and experience; for that rubber anſwers beſt for an uſual electrical machine, which can eaſily conduct the electric fluid with its under part, and the upper part of which is more ready to acquire, and to retain, the negative electricity; hence the rubbers are generally furniſhed with amalgam below, and with a piece of ſilk above; hence alſo, if the cylinder of the machine be turned the contrary way, it will produce little or no electricity.

It often happens, that the part which conducts the fluid, and that which acquires the electricity contrary to that of the electric, are not ſo diſpoſed in a rubber as the one above deſcribed; but it remains always true, that the rubber muſt be poſſeſſed of thoſe two properties, *viz.* to conduct the electric fluid very readily in one or more parts, and to acquire, as well as retain, on other parts, an electricity contrary to that acquired by the electric that is to be rubbed with it.

Little St. Martin's-Lane,
Nov. 7, 1787.

T. CAVALLO.

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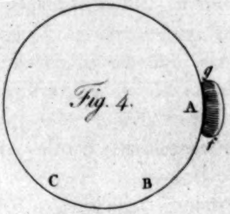
Fig. 4.

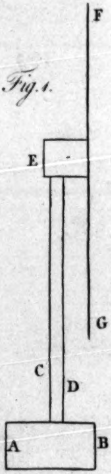
Fig. 1.

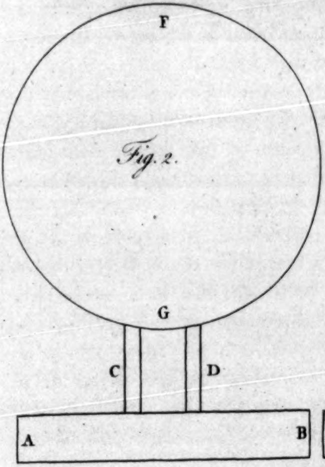
Fig. 2.

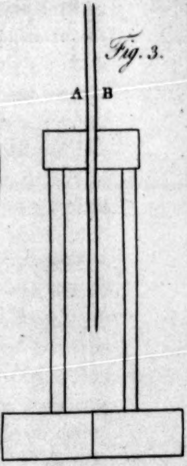
Fig. 3.

T. Cavallo del.

II. *The Croonian Lecture on Muſcular Motion.*

By George Fordyce, *M. D. F. R. S.*

Read November 22, 1787.

THE ſubject of muſcular motion has been ſo often conſidered, and in ſo many lights, that it is hardly poſſible to avoid many obſervations which are trite, and even puerile; which, when they occur, I beg the learned Society to forgive. They will alſo find ſo little new, or even nothing, that I ſhould be inexcuſable in taking up their time, if the ſubject was not annually to be diſcuſſed before them. In conſidering muſcular motion, I muſt begin with ſome obſervations on motion in general, and with that well known, and ſelf-evident axiom, that one particle of matter, conſidered by itſelf, will remain at reſt if it be at reſt, and will continue in motion if it be in motion, and in the ſame direction. This has been called the *vis inſita*, or *vis inertiæ*, of matter. It may be ſaid in other words, that a ſingle particle of matter being at reſt, would therefore always continue at reſt, if it were not for ſome external impulſe made on it. This impulſe may be from ſome other particle of matter in motion impinging upon it, and communicating part of its motion to it, while it communicates an equal quantity of its reſt to the matter ſo impinging upon it, ſo that the quantity of motion and reſt ſhall be the ſame after the impinging, in both bodies, as they were before: or, in other words, a ſimple particle of matter in motion would always continue in motion, in the ſame direction, if it did not meet with another, on which it impinged; and after the impinging, there would be the ſame quantity of motion and reſt in both bodies taken together, as was before. If we conſider equal motion, in direct contrary direction, as reſt; motion, or reſt, produced in a body by the above means, I ſhall call communicated.

If two ſimple particles of matter, of any ſpecies, not farther diſtant from one another than the ſun is from the earth, were both at perfect reſt, theſe two particles would inſtantly begin to move toward one another, if no other particle of matter whatever exiſted.

I do not mean to tire my learned Audience with demonſtration of propoſitions ſo well known.

There would, therefore, be an impulſe, producing motion between theſe bodies, without any contact.

Motions produced in this way, I call original motions.

The firſt conſideration with regard to any particular motion is, therefore, whether it be an original or communicated motion. If it be an original motion, it will follow the laws of that particular ſpecies of original motion; if it be a communicated motion, it will follow the laws of communicated motion.

Many obſervations ſhew, that muſcular motion is not a communicated motion, and therefore an original one.

In any ſyſtem of bodies, or particles of matter, affecting one another only by the motions already exiſting in them being communicated to one another, they may diminiſh their motion, or bring one another to reſt; but they never can increaſe the motion exiſting in the whole. It happens frequently, that the motions in the animal body are increaſed, without any alteration of external applications to it; the caſes are ſo numerous, that it is hardly worth bringing an example: we might mention the increaſe at times of the circulation, and all the motions of the fluids without the leaſt new motion in the ſurrounding bodies, or interference, or even knowledge of the mind. This motion muſt therefore be original, and not communicated.

In communicated motion, if one body be at reſt, and a motion be communicated to it by another, the power of the whole motion ſhall not be greater than that in the communicating body at the time of the communication. If I take out the heart of an animal, cut off the auricles, it will in many caſes continue to contract and dilate for ſome time. If it be left to come to reſt, and if ſoon after a needle be introduced into the ventricle, placed tranſverſely, and if the interior ſurface of the ventricle be pricked gently by the needle, the ventricle will contract with ſuch power as to force the needle deep into it: in this caſe, the force of the contraction of the ventricle is much greater than the power with which it was pricked by the needle; this contraction was therefore not communicated to it by the moving needle, but was generated, and therefore an original motion.

In all communicated motion, by which two bodies at a diſtance are brought near to one another, there muſt ſubſiſt ſome other matter, by which they may be drawn, or forced, nearer to one another; but in original motion, it is not at all neceſſary that any other matter ſhould exiſt at all. Two particles, placed at as great a diſtance from one another as the ſun is from the earth, as has been already obſerved, although at perfect reſt, would begin to move nearer one another, by the attraction of gravitation, if all other matter whatever were annihilated.

Moſt authors who have treated on muſcular motion have ſuppoſed, that it was a communicated motion; and that it was produced by ſomething paſſing by the nerves, from the brain to the moving part. Three doctrines have been ſet forth; one, that there is a fluid paſſing along the nerves; a ſecond, that there is a vibration; and a third, that the nerves are ſurrounded with ſomething like electric matter, in which motion runs from the brain to the moving parts. Thoſe who have conſidered this ſubject muſt be tired of the arguments which have been brought to refute each of theſe; for no argument from fact has been employed to prove any one of them: I ſhall therefore leave them as mere chimeras of the brain. I have taken notice, that it is not requiſite for any motion to paſs between two bodies, exciting in each other an original motion, through or by any other matter: I have alſo ſhewn, that muſcular motion is an original motion: it follows, that it is not neceſſary for any motion, or communication, to paſs through any other matter, in order to bring the muſcular fibres into action.

One caſe of muſcular motion is, when a ſtimulus is applied to ſome part of the body, and a muſcle at a diſtance immediately contracts. It has been ſuppoſed in this caſe, that ſome influence was communicated to the nerve of the part where the ſtimulus was applied, and through it to the brain, and from the brain through the nerves of the contracting muſcle. Granting, for a little, that ſome motion may paſs along the nerves, and therefore that the end of the nerve, where the application was made, may be the part in which the original motion began, the ſtimulus frequently does not touch the end of the nerve; for if vapour of volatile alkali be applied to the noſtrils, an univerſal glow of heat, and increaſed circulation, will conſtantly take place; but the vapour of the volatile alkali could not touch the nerves of the noſtrils, the membrane being conſtantly covered with mucus, which the vapour could not penetrate without diſſolving it, which it had not time to do, and, if it had, would have united with it, ſo as to form a ſoap, void of any ſtimulating power.

If, therefore, the original motion began in the end of the nerves of the noſtril, it muſt be excited by a ſubſtance at a diſtance from the end of that nerve, without any communication of motion between the ſtimulus, that is, application producing the motion, and the end of the nerve in which it is excited; therefore, on any ſuppoſition, a ſtimulus is capable of exciting a motion, in a part at a diſtance, without any communication of motion; and it is therefore not neceſſary, that the nerves ſhould be at all employed in the motions of the body excited by a ſtimulus, as it can act at a diſtance without their intervening. Further, that the nerves are not employed in the motions excited by ſtimuli, is evident from this experiment: take the heart out of a living animal, cut all the nerves off as cloſe as poſſible, lay it in nearly the heat of the body of the animal, it will continue to contract for ſome time. As ſoon as it has ceaſed contracting, prick a fibre in one of the ventricles; both ventricles, and all their fibres, will contract inſtantly, although there be now no communication by the nerves, between many of the contracting fibres, and the fibre ſtimulated. It might be ſuſpected, that the motion of the fibre ſtimulated might affect the others: in this caſe the contractions would be progreſſive; but, on the contrary, the whole contract at once.

I cannot help bringing another inſtance, where ſtimuli produce action in parts at a diſtance, without any communication of motion by the nerves. When infuſion of cantharides is applied to the ſkin, as we ſay vulgarly, it is not applied immediately to the ſkin; but in the firſt inſtance to the mucous and ſebaceous matter, which every where covers the ſcarf ſkin; under this lies the ſcarf ſkin, which the infuſion can hardly be conceived to come at; if it did, the ſcarf ſkin we know is perfectly impenetrable to ſuch a fluid; it can therefore never touch the ſkin, in which it excites inflammation, and on which it therefore acts at a diſtance, and excites motion, which no one can ſuſpect to come through the nerves: nor is there any motion through the mucus and ſcarf ſkin, of any other kind than would ariſe if an infuſion of any other inſect had been applied, which had no power of exciting inflammation.

From what has been ſaid we may conclude, that when a ſtimulus has been applied ſo as to excite motion in a diſtant part, no motion whatever takes place in the nerves, or is communicated by them from the part to which the ſtimulus is applied to the moving part.

I need not draw your attention to another propoſition, *viz.* that when a ſtimulus is applied to a diſtant part, ſo as to produce motion, it often happens, that the ſtimulating matter is not carried by the blood-veſſels, or otherwiſe, to the moving part. This propoſition has often been demonſtrated, and is well known.

All the original power exerted by any of the moving parts conſiſts in a power of particles coming nearer one another; for every muſcle or fibre becomes ſhorter when it acts; or, in other words, contracts; and every other moving part in like manner contracts when in action. It is true, that there are many contrivances to make the contraction have great effect in producing motion, force being never ſpared for conveniency, as my friend Mr. HUNTER has, I believe, already ſet forth to my learned audience: but, nevertheleſs, it is clear from every experiment made on the ſubject, that all motion ariſes from particles coming near one another in ſome direction. It would be ſuperfluous for me to point out theſe experiments; I ſhall mention one only, and an obvious one. Lay bare a muſcle, and prick any of its fibres, it immediately becomes ſhorter.

The original power of coming nearer to one another of two or more particles of matter, has been called attraction. There have been ſeveral original powers of coming nearer one another of particles of matter, which have been conſidered as different attractions, ſuch as the attraction of gravitation, of magnetiſm, of electricity, &c.; all of which this Society are too much maſters off, for me to enter into any diſquiſition with regard to them.

The attraction which is my preſent object, I call the attraction of life. This attraction is either of two ſpecies, or is exerted variouſly; for all the moving parts have their particles nearer one another in the living than in the dead body. The proof of this is as neceſſary as it is obvious. I muſt, therefore, take up a ſmall portion of your time in pointing it out. Take the body of any animal, when the life is entirely gone from it, and the effects of it are entirely loſt, but before any putrefaction, or any change in its chemical qualities, has taken place; and lay bare, and diſſect out, any muſcle, eſpecially one which has long fibres, and no middle tendon, ſuch as the *ſartorius*, for example, and afterwards lay it in its place, leaving it of the length it naturally takes; it will reach farther than from its origin to its inſertion; but lay bare, and diſſect out, the ſame muſcle in the living body, and it will always be ſhorter than from its origin to its inſertion. If it ſhould be ſaid, that the diſſection ſtimulated the muſcle, and brought it into action, let it not be diſſected out, but its tendon cut through, as the

Tendo Achillis, for inſtance, the ſame thing will happen. And we now are all convinced, from various experiments, that a tendon in a ſound ſtate is not capable of being ſtimulated by being wounded, cut through, or broken.

I apprehend, then, that we may conclude, that all the moving parts are conſtantly contracted, that is, their particles are nearer one another when the body is alive, than when dead, and totally left to their elaſticity. This ſpecies of action I call the tone. The ſecond ſpecies, or variety, which occurs in the attraction of life, is when a moving part, for a ſhort time, has its particles brought nearer one another than they are from their tone, and which very rarely continues for many ſeconds of time without intermediate relaxation. I call it their action; when it does continue for a longer time, it is called ſpaſm; which, however, is ſo vague a term, that I could wiſh totally to reject it, at leaſt to confine it only to this ſenſe, *viz.* a greater contraction, or coming nearer one another of the particles, of a moving part, than that which would happen from their tone, remaining without any intermediate relaxation.

I for the preſent do not mean to ſay any thing farther with regard to the tone, or ſpaſm of parts; but only to call to your conſideration the action, as excited by applications to ſome part of the body at a diſtance from the moving part. I have already rejected all communication by the application, or ſtimulus, being carried by the blood-veſſels, or any other way whatever, to the part. I have alſo rejected any motion, or communication of any kind whatever by the brain and nerves to the part. I conceive, that when any ſtimulus or application whatever is made in any part, ſo as to produce any action in a diſtant part, that that medicine or application, without having any operation whatever on the intermediate parts, gives a power to the particles of the moving part of greater attraction. I ſhall illuſtrate this idea by ſuppoſing, that there is a machine moving by various powers, either original or communicated; and that in this machine there are two magnets, which by their attractive power have come to a given diſtance from one another, but have been prevented from coming nearer by ſome power endeavouring to draw them back. A much ſtronger magnet applied to a part of the machine, in a certain manner, ſo as not to touch either of the two already there, nor to affect any other part, may increaſe their power of attraction, ſo as to make them overcome the reſiſtance, and come nearer one another *.

In the ſame manner, I apprehend, that an application made to the ſkin of the abdomen may, and often does, occaſion the action of the inteſtines to take place, without any effect whatever on the intermediate parts; but that it ſimply excites the attraction of the particles of the moving parts of the inteſtines; certainly a part of the matter through which the influence is to paſs, *viz*, the mucus and ſcarf ſkin, is actually inanimate matter.

In certain caſes of original motion, there is attraction, or the coming nearer of particles only. In others, there is not only attraction, but oppoſite repulſion; the caſes of which are unneceſſary to enumerate to this Society. In the attraction of life there is no oppoſite repulſion; all the motions of the body are produced entirely by the force of particles coming nearer one another. When this force diminiſhes, or the action goes off, and leaves the part entirely to its tone, the particles of the moving part are by no means repulſed from one another, but are left to be drawn aſunder by their elaſticity, weight, or the weight of the ſurrounding parts, or any other accidental power: but, nevertheleſs, there are applications which may be made to diſtant parts of the body, which may, and do, take off the attraction which occaſions the action of the moving part; and all thoſe reaſonings which I have already applied to applications which excite action, and which are called ſtimuli, are equally applicable to thoſe applications which make action ceaſe, and which we call ſedatives.

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* I do not mean to inſinuate, in the ſmalleſt degree, that the powers of the body at all depend on, or have any thing to do with, magnetiſm.

The great ground on which I have attempted to make theſe obſervations, is the foundation of certain maxims in the practice of medecine, which I ſhall now proceed to ſketch out to the Society, whoſe inſtitution, to me, has always ſeemed to include the philoſophy, but not the actual practice, of medicine.

Medicine is a ſcience of long cultivation in that channel in which all the ſciences have flowed, and had early attained great perfection, I believe, from the teſtimony of various writers of antiquity, and other circumſtances, which I paſs over as well known to many of this learned body, and too foreign a digreſſion from our preſent ſubject: for although CELSUS obſerves well, that there could be no phyſicians among the Greeks at the time of the Trojan war, inaſmuch as HOMER never mentions one medicine, but only application to the Gods for the cure of fevers, and other internal diſeaſes; yet the Egyptians, from whom the Greeks received a great part of their knowledge in all ſcience, as well as in medicine, had certainly not only regular phyſicians for internal diſeaſes, but likewiſe ſtone-cutters, oculiſts, auriſts, &c. long before the Trojan war; and HIPPOCRATES, by his own teſtimony, took much of his knowledge from what he calls the ancients. In the progreſs, therefore, of the ſcience of medicine, it came into my mind to enquire how far, and on what ground, the modern increaſe of ſcience in anatomy, chemiſtry, mathematics, &c. had forwarded the knowledge of medicine. In the firſt place, it is well known to my learned Audience, that medicine was in the hands of Greek phyſicians from the time of HIPPOCRATES, or rather from the deſtruction of the Egyptian monarchy by CAMBYSES, down to the time of the Cruſades; in all this time there was hardly a diſſection of the human body, from an opinion about *manes*; but when it came into Europe again, where this opinion remained indeed, but in a much leſs degree, anatomy began again to flouriſh; and by other means all the other ſciences ſhone forth with a greater luſtre than they had ever done in any period handed down to us by hiſtory of any nation. It was obvious therefore to conceive, that the knowledge of the ſtructure of the body, and the inveſtigation of the powers of matter, made in a more accurate manner, and on a more extenſive ſcale, would elucidate the doctrine of the human body, and its diſeaſes, and their treatment, in a new and more perfect manner: to this opinion I mean now to apply the reaſoning I have before laid down.

In the ſtructure and phyſiology of the body, two great diſcoveries have been made by the moderns; the circulation of the blood; and the lymphatics, and abſorption of the lymph. Theſe at once overthrow the ideas of the ancients with regard to moſt of the functions of the interior parts of the body; which was now conceived by many to be an hydraulic machine, and ſubject to all thoſe diſorders which were incidental to ſuch a machine; and particularly, from various fluids flowing with great rapidity, through tubes, many of them of infinite fineneſs, that ſtoppages muſt often take place, which

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were to be removed by diſſolving out the obſtructing matter; that the blood was mixed ſo perfectly through all the body, and ſo conſtantly, that the ſame blood muſt be taken away, whatever blood-veſſel was opened; and when we contemplate the numerous openings and communications of the veſſels with one another, that blood flowing out of any one will empty them all equally, and that, therefore, it can be of no conſequence from what part of the body blood is evacuated. In a pleuriſy, for inſtance, where can be the difference, whether blood be taken from the right or left arm, or from the veſſels of the ſkin of the breaſt? But there is a difference, and a great one too; ſince taking a much leſs quantity of blood from the ſkin of the breaſt, is actually known, in certain caſes, from experience, to cure a pleuriſy, than would have had that effect if taken from the veſſels in the arm, and will even carry off the diſeaſe, when it could not be carried off at all by evacuation from the arm: yet it is undoubtedly the very ſame blood in all its qualities; and in both caſes the veſſels of the pleura are equally emptied. The act of flowing out of the blood from the veſſels of the ſkin of the breaſt then has an immediate action on the action of the moving parts in the pleura, and carries off the inflammation independent of the circulation, or any of its laws; and ſo far has the knowledge of the circulation been of any advantage in this caſe, that it had nearly thrown out topical bleeding in inflammation, which is one of our moſt powerful remedies in the diſeaſe. In like manner, when the moving fibres of the ſtomach do not contract, ſo as to expel any vapour that may get into it, a ſpice applied to the ſkin over the ſtomach will, in many caſes, occaſion theſe fibres to contract. Now it is well known from anatomy, that there is no communication between the ſkin of the abdomen and the ſtomach; and if the ſpice were to act by touching theſe fibres, it would be the ſame, whether it was applied to the ſkin over the ſtomach, or to the ſkin of the arm; for in both caſes it muſt be abſorbed by the lymphatics, and carried to the left ſide of the heart, and there and in the lungs be blended univerſally with the whole blood, and carried by the arteries to the moving fibres of the ſtomach. Nay more, that it would be equal, whether it was applied to the inner ſurface of the ſtomach itſelf, or to the external ſkin, or any other membrane, of any other cavity: for the ſtomach is covered with mucus, and lined with a membrane which is perfectly impervious, and totally prevents any thing contained in the ſtomach from being any way applied to the moving fibres; it muſt in this caſe, therefore, be likewiſe taken up by the lymphatics, or lacteals, and carried to the heart before it could touch the moving fibres of the ſtomach. The maxim then, ariſing from our knowledge of the lymphatics, would be, that it was of no conſequence where we applied a ſpice in caſes of flatulency; which is not true. By ſimilar reaſons it might be eaſily ſhewn, were it not for tiring my learned Audience, that all the knowledge of the properties of the fluids, which has been acquired by modern and accurate experiments, hardly contributes any thing to the knowledge of applying medicines for the cure of diſeaſes; and that the ſtudy of the laws of the attraction of life, or what has been called muſcular motion, is of conſiderable importance.

One more obſervation I have only to make, *viz.* that all original motions are by their nature perfectly unintelligible as to their cauſe; who can tell the cauſe of gravity, chemical attraction, &c.? and ſo undoubtedly the attraction of life, in its cauſe can never be inveſtigated, being, like all other attraction, a power which two or more particles of matter have of coming nearer one another. But although the ſtudy of cauſes of original powers be totally abſurd and futile, yet the laws of their action are capable of inveſtigation by experiment, and applicable to the evolving much uſeful knowledge. Need I remind this Society, that the inveſtigation of the laws of gravitation, by Sir ISAAC NEWTON, has rendered it immortal? The inveſtigation of the laws of the attraction of life has alſo been greatly forwarded by one of its preſent members; but it is an inveſtigation which will probably require ages to render perfect.

III. *An Account of a Maſs of native Iron, found in South-America. By Don* Michael Rubin de Celis. *Communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read November 22, 1787.

Memoria que remite a la R. Sociedad de Londres D. MIGUEL RUBIN DE CELIS, Cavellero del Orden de Santiago, Teniente de Fragata de la R. Armada de S. M. C. Academico correſpondiente de la R. Academia de Marina de Francia, y de la R. de la Hiſtoria de Eſpaña.

HAZE mas de treinta años que en las provincias del gran Chaco Gualamba, de adonde varias Naciones barbaras arrojaron a los Eſpañoles, y deſde cuio tiempo eſtà quaſi deſierta la parte meridional del Rio Bermejo, y occidental del gran Paranà, los pocos Indios que ſe hallan en la juriſdicion de Santiago del Eſtero, teniendo por unico exercicio el ſacar Miel y Cera de los montes, de la que hay una cantidad prodigioſa en los arboles, deſcubrieron en el medio de un campo un trozo de metal que llamaron fierro puro, y que ſobreſalia de la tierra por una banda coſa de un pie, y ſe veìa quaſi toda ſu cara ſuperior: anuncioſe a los Virreyes del Peru, como un deſcubrimiento raro, y con razon al parezer, pues donde no hay montañas, ni la mas pequeña piedra en cien leguas al rededor, una coſa ſemejante merezia el nombre de maravilloſa, ſin embargo de conſtar haber en Europa minas de fierro puro. Hubo particulares que con rieſgo eminente de ſus vidas, ya por la eſcaſez de aguas, que no hay ſino la que en tal qual cavidad natural ſe recoje de las llubias, ya por la eſcazez de viveres, ya por el rieſgo de los Indios barbaros, ya por los varios animales ferozes, como Tigres, Leopardos y Antas, ya por los muchos reptiles ponzoñoſos, ya enfin por las infinitas eſpeſuras, ſe arrojaron, quiza ſeducidos de la codicia, à hazer la trabeſia para extraher algo de la materia: remitieron de ella à Lima y Madrid, donde no ſe adelantò otra coſa que ponerla el nombre de fierro mui dulze y mui puro. Como que el fierro, por razon politica, no ſe trabaja en aquel continente, aunque de el hay muchas minas, y como que ſe aſeguraba que la veta del fierro corria muchas leguas, ſiendo aſi que lo viſto ſolo era un creſton ſobreſaliente à la tierra, y que deſpues de eſcavado tenia tres varas de N. à S. dos y media de E. al O. y una terzia de alto, me deſtinò el Virrey del Rio de la Plata à que examinaſe prolijamente la dicha maſa, y en el caſo de ſer mina, y util, proyectaſe ſobre Poblacion. Sali con competente eſcolta, a principios de febrero de ochenta y tres, del Rio Salado, antigua reduccion de los Indios Vilelas, ſiguiendo el Rumbo de E. ¼ N.E. bien que debi ſeguir el del E. ¼ S.E. todo corregido.

El aſpecto del pais que media entre el Rio y la Mina, diſtante ſetenta leguas de la Reduccion, es mui curioſa; una llanura immenſa, alternada de montes eſpeciſſimos y campos fertiles, forma la mas agradable perſpectiba.

Obſervè la latitud de la mina 27 gr. y 28 min.

No hay Poblacion alguna en toda la traveſia, por eſcaſez de aguas manantiales: la que gaſtan los meleros, que en pequeñas caravanas reſiden la maior parte del año meleando en los montes, es la llovediza como ſe ha expreſado. Eſtos y algunas tribus de Indios errantes y barbaros, cuia vida es ſemejante a la de los Tartaros, que en cierta eſtacion del año vienen de las marjenes del Rio Vermejo à cabar o ſacar una raiz ſilveſtre llamada Koruu: que ſolo la maſcan continuamente, ſiendo para ellos de primera neceſſidad, aſi para preſerbarſe de los efectos de una atmoſphera infeſtada, como de las mordeduras continuas de los reptiles ponzoñoſos : ſon las unicas gentes que ſe hallan en eſtos dilatadiſſimos y amenos Campos.

El quinze lleguè a los nombrados de Otumpa, donde ſe hallava la maſa quaſi enterrada en pura greda y cenizas.

La apariencia exterior era de fierro perfectamente compacto ; no aſſi la interior, como puede verſe en las cavidades que manifieſtan los cortes de los pedazitos, lo que induze à creer haber eſtado liquida en algun tiempo : fortifica eſta idea una porcion de trazas exteriores en la parte ſuperior ; ſe hallan las de pies y manos de Hombres de crecida eſtatura, las de pies de varios paxaros grandes communes en aquellos campos : y aunque perfectas las tales trazas no dejo de perſuadirme a una de dos coſas, obien que ſea un capricho ò accidente natural, obien que en el ſuelo, antes de caer alli aquella maſa, exiſtieren las tales huellas, y que en la caida de la maſa liquida hubieſen reſultado en la parte ſuperior. A nada ſe aſemeja tanto como a un amaſijo de pan que, deſpues de haberſe en el eſtampado pies manos y metido un dedo, ſe hubieſe ferrificado.

Empezé à cortar con los cinceles, y para ſacar veinte y cinco, ò treinta libras, fue neceſſario romperlos todos en numero de ſetenta.

Hize cavar al rededor del trozo, quien por la parte inferior tenia una capa del grueſo de quatro ò ſeis pulgadas de eſcoria, procedida de las humedades de la tierra ; por la ſuperior eſtava limpio.

Deſprendido de la tierra, le hize dar una media buelta con palancas, y cabè en la cama a baſtante profundidad. No contento conſtruì dos hornillos, que volè, y con cuidado examinè la tierra mas profunda, y la hallè perfectamente ſemejante a la ſuperior, a la de todos aquellos campos, y a la de dos pozos que hize abrir a diſtancia de ſeſenta ò cien paſos del trozo y direccion del E. O.

El no encontrar raiz ni ſerie de generacion, avivò mas la admiracion mia, y empeze a diſcurrir de eſte modo.

O eſta maſa ſe crio aqui, o fue traìda ò arrojada? Criarſe aqui no es poſible, ſegun el conocido proceder de la Naturaleza: ſer trahida de donde, para que, ò como? de donde, no haviendo en centenares de leguas minas de fierro, ni memoria de que ſe hubieſen trabajado en el Reyno? para que? ſiendo de una materia ſin eſtimacion, no pudiendoſe uſar, y ſiendo aquel un terreno el mas eſcuſado o inavitable de todo el Chaco, por la eſcaſez de aguas; como? no teniendo noticia de que los Indios hayan conocido jamas las maquinas de acarreo; con que habrà ſido arrojada por algun volcan.

Las razones que induzen a creer eſto ſon varias. Regularmente los volcanes dejan deſpues de volados algunos pozos de agua, unas vezes termales, y otras frias: al E. del trozo, y diſtancia como de dos leguas, ſe halla vna aguadita baſtante ſalobre vnica y manancial por todo aquel pais. En todo lo que caminè del Chaco no obſerbè a la viſta diferencia ninguna de nivel en el terreno: ſolo en la imediacion à eſta aguadita hay vna rampa que corre N. S. y que por particular ſeme hizo notable antes de ver el trozo. Graduando poder tener de diferencia de nivel con el reſto del pais de quatro ò ſeis pies: las imediaciones a la papa ò trozo, y a la aguadita, eſtan cubiertas de una eſpecie de tierra menuda como la ceniza, y quaſi de ſu color: el Paſto de eſtas imediaciones, llamado Ahuvi, es ſumamente pequeño, delgado, y aborrecido de los animales, aſi como mui alto y mui grato a ellos el que eſtà algo ſeparado del trozo y de la aguadita: à pequeña profundidad en eſta ſe encuentran unas piedras quarzos de un encarnado hermoſo, las que han uſado los meleros ſolo para encender fuego : hay noticia de haber aquellos trahido tres pequeñas por particulares en ſu color, y unas pintas doradas ; una de ellas del peſo de una onza vino a poder del Governador de Armas de Santiago del Eſtero, quien me dijo haberla echo moler, y me moſtrò mas de un adarme de oro que de ella ſe havia extraido. Es indudable que entre aquellos immenſos boſques exiſte un Arbol con ſus ramas de puro fierro; ſon muchos los Indios que le han viſto, y los de la reduccion llamada de Avipones ſaben ſu verdadero lugar: un Europeo diſtinguido vezino de la Cuidad de Salta lo ha tocado: la caida o inclinacion del trozo es del E. al O. haviendo dejado vna eſpecie de corte al E. cuia direction es N. S. de eſte corte es de donde ſe pudieron extraer con cinceles los pedazitos de metal: luego con eſtos antecedentes no es violento formar eſta Ypoteſi.

Voloſe el volcan en la aguadita ſalobre, lebantò vna porcion de tierras que por ſu poca gravedad ſe quedaron en la imediacion ; aunque al principio amontonadas, las continuas aguas del Chaco, que ſe halla inundado la terzera parte del año, las ha ido igualando con el otro terreno: la direccion de la materia grande arrojada fue del E. al O. y como peſada alcanzò a la diſtancia en que ſe encuentra: alguna otra porcion de materia mas pequeña, y acaſo mas fluida, ſe ſeparò a otro rumbo, haciendo canales, al modo que quando ſe arroja un baſo de agua; eſta pequeña materia ya fria, y lamida con las aguas la tierra que al principio la ſoſtenia, devio de formar lo que hoy ſe llama arbol de fierro: las materias ſalitroſas y antimoniales, que accompañan de ordinario à todo mineral, devieron igualmente eſparcirſe y infertelizar los contornos.

En el Reyno de Santa Fee de Bogotà ſe encuentra la Platina en polbo meſclada con el oro: pocos ignoran la mucha afinidad que tienen eſtos dos metales, con que no ſeria extraño, ſegun el conjunto de todas eſtas razones, que el fuego del volcan hubieſe derretido la Platina, y arrojadola como materia ſuperior a la de oro, que era inferior. Eſte principio de los volcanes es el mas natural para comprobar el porque ſe hallan tambien ſueltas las afamadas papas de Plata de Guantajaia, ſobre cuia formacion ſe han dicho exquiſitas extravagancias y ridiculezes.

Cubicado ſobre poco mas o menos, reputando la gravedad expecifica del metal algo maior como lo es que la del fierro, vendrà a peſar mas de treſcientos quintales.

El Cav. MIG. RUBIN DE CELIS.

R. Iſla de Leon,
2 de Junio de 1786.

Some ſpecimens of the Iron accompanied this Paper, and were laid before the Society; who afterwards preſented them to the Britiſh Muſeum. C. B.

IV. FRIGORIFIC EXPERIMENTS ON THE MECHANICAL EXPANSION OF AIR, *explaining the Cauſe of the great Degree of Cold on the Summits of high Mountains, the ſudden Condenſation of aerial Vapour, and of the perpetual Mutability of atmoſpheric Heat. By* Eraſmus Darwin, *M. D. F. R. S.*; *communicated by the Right Honourable* Charles Greville, *F. R. S.*

Read December 13, 1787.

HAVING often revolved in my mind the great degree of cold producible by the well known experiments on evaporation; in which, by the expanſion of a few drops of ether into vapour, a thermometer may be ſunk much below the freezing point; and recollecting at the ſame time the great quantity of heat which is neceſſary to evaporate or convert into ſteam a few ounces of boiling water; I was led to ſuſpect, that elaſtic fluids, when they were mechanically expanded, would attract or abſorb heat from the bodies in their vicinity; and that, when they were mechanically condenſed, the fluid matter of heat would be preſſed out of them, and diffuſed among the adjacent bodies.

As this principle might poſſibly be extended to elaſtic ſolid bodies, as well as to fluid ones, and explain the cauſe of the heat occaſioned by percuſſion or friction, and by ſome chemical combinations, as well as the perpetual mutability of it in the atmoſphere, I have, at different times, endeavoured to ſubject it to experiment.

1. When Dr. HUTTON of Edinburgh, and Mr. EDGEWORTH of Edgeworthtown in Ireland, were with me about twelve or fourteen years ago, the following experiment, which had been propoſed by one of the company, was carefully made. The blaſt from an air-gun was repeatedly thrown on the bulb of a thermometer, and it uniformly ſunk it about two degrees. The thermometer was firmly fixed againſt a wall, and the air-gun, after being charged, was left for an hour in its vicinity, that it might previouſly loſe the heat acquired in the act of charging; the air was then diſcharged in a continued ſtream on the bulb of the thermometer, and the event ſhewed, that the air at the time of its expanſion attracted or abſorbed heat from the mercury of the thermometer.

In March 1785, by the aſſiſtance of Mr. FOX and Mr. STRUTT, of Derby, a thermometer was fixed in a wooden tube, and ſo applied to the receiver of an air-gun, that, on diſcharging the air by means of a ſcrew preſſing on the valve of the receiver, a continued ſtream of air, at the very time of its expanſion, paſſed over the bulb of the thermometer. This experiment was four times repeated in the preſence of many obſervers, and uniformly ſunk the thermometer from five to ſeven degrees. During the time of condenſing the air into the receiver, there was a great difference in the heat, as perceived by the hand, at the two ends of the condenſing ſyringe; that next the air-globe was almoſt painful to the touch; and the globe itſelf became hotter than could have been expected from its contact with the ſyringe. Add to this, that in exploding an air-gun, the ſtream of air always becomes viſible, which is owing to the cold then produced precipitating the vapour it contained; and if this ſtream of air had previouſly been more condenſed, or in greater quantity, ſo as not inſtantly to acquire heat from the common atmoſphere in its vicinity, it would probably have fallen in ſnow, as in the fountain of Hiero, mentioned below.

2. About twelve or fourteen years ago, by the aſſiſtance of Mr. WALTIRE, a celebrated itinerant teacher of philoſophy, a thermometer was placed in the receiver of an air-pump, and ſome time being allowed, that it might accurately adapt itſelf to the heat of the receiver, the air was haſtily exhauſted; during which the mercury of the thermometer ſunk two or three degrees, and after ſome minutes regained its previous height. In November 1787, by the aſſiſtance of my very ingenious friend Mr. FORESTER FRENCH, the above experiment was repeated; but with this difference, that the thermometer was open at the top; ſo that the diminution of external preſſure could not affect the dimenſions of the bulb; and the reſult was the ſame, the mercury in the thermometer ſunk two or three degrees, and gradually roſe again. Does not this ſhew, that the air in the receiver, being expanded during the exhauſtion, attracted or abſorbed heat from the mercury in the thermometer?

Both during the exhauſtion, and during the re-admiſſion of the air into the receiver, a ſteam was regularly obſerved to be condenſed on the ſides of the glaſs, which in both caſes was in a few minutes re-abſorbed. This ſteam muſt have been precipitated by its being deprived of its heat by the expanded air: if it could have happened from any other cauſe, the vapour could not, in both ſituations, *viz.* of exhauſtion, and of re-admiſſion, have been taken up again.

3. In December 1784, with the aſſiſtance of Mr. Fox, the following experiment was carefully made. A hole, about the ſize of a crow-quill, was bored into a large air-veſſel, placed at the commencement of the principal pipe in the waterworks which ſupply the town of Derby. The water from four pumps, which are worked by a water-wheel, is firſt thrown into the lower part of this air-veſſel, and from thence riſes to the top of St. Michael's Church into a reſervoir, which may be about thirty-five or forty feet above the level of the air-veſſel.

Two thermometers were previouſly ſuſpended on the leaden air-veſſel, that they might become of the ſame temperature with it; and, as ſoon as the hole was opened, had their bulbs reciprocally applied ſo as to receive the ſtream of air; and the mercury in both of them ſunk two diviſions, or four degrees. This ſinking of the mercury in the thermometers could not be aſcribed to any evaporation of moiſture from their ſurfaces, becauſe it was ſeen, both in exhauſting and re-admitting the air into the exhauſted receiver, that the vapour which it previouſly contained was depoſited during its expanſion.

4. There is a very curious phænomenon obſerved in the fountain of Hiero, conſtructed on a very large ſcale in the Chemnicenſian mines in Hungary, which is very ſimilar to the experiments above related. In this machine the air, in a large veſſel, is compreſſed by a column of water 260 feet high: a ſtop-cock is then opened, and as the air iſſues out with great vehemence, and, in conſequence of its previous condenſation, becomes immediately much expanded, the moiſture it contained is not only precipitated, as in the exhauſted receiver abovementioned, but falls down in a ſhower of ſnow, with icicles, adhering to the noſel of the cock. This remarkable circum-

ſtance is deſcribed at large, with a plate of the machine, in the Philoſophical Tranſactions for 1761, Vol. LII.

5. From the four experiments already related; firſt, of the mercury ſinking in the thermometer, by being expoſed to the ſtream of air from an air-gun; ſecondly, from its ſinking in the receiver of an air-pump, during the time of exhauſting it; thirdly, from its ſinking when expoſed to a ſtream of air from the air-veſſel of a water-engine; and, laſtly, from the curious phænomenon of ſnow and ice being produced by the ſtream of expanding air from the fountain of Hiero in an Hungarian mine; there is good reaſon to conclude, that in all circumſtances, when air is mechanically expanded, it becomes capable of attracting the fluid matter of heat from other bodies in contact with it.

Coldneſs of the Summits of Mountains.

Now, as the vaſt region of air which ſurrounds our globe is perpetually moving along its ſurface, climbing up the ſides of mountains, and deſcending into the vallies; as it paſſes along, it muſt be perpetually varying its degree of heat, according to the elevation of the country it traverſes: for in riſing to the ſummits of mountains it becomes expanded, having ſo much of the preſſure of the ſuper-incumbent air taken away, and, when thus expanded, it attracts or abſorbs heat from the mountains in contiguity with it; and when it deſcends into the valley, and is again compreſſed into leſs compaſs, it again gives out the heat it has acquired to the bodies it becomes in contact with.

The ſame thing muſt happen in reſpect to the higher regions of the atmoſphere, which are regions of perpetual froſt, as was always ſuſpected, and has of late been demonſtrated by the aërial navigators. When large diſtricts of air from the lower parts of the atmoſphere are raiſed two or three miles high, they become ſo much expanded by the great diminution of the preſſure over them, and thence become ſo cold, that hail or ſnow is produced from the precipitated vapour, if they contain any: and as there is, in theſe high provinces of the atmoſphere, nothing elſe for the expanded air to acquire heat from, after the precipitation of its vapour, the ſame degree of cold continues, till the air, on deſcending to the earth, acquires again its former ſtate of condenſation and of warmth.

The Andes, almoſt under the line, reſts its baſe on burning ſands; about its middle height is a moſt pleaſant and temperate climate, covering an extenſive plain, on which is built the city of Quito; while its forehead is incircled with eternal ſnow, coeval perhaps with the elevation of the mountain: yet, according to the accounts of ULLOA, theſe three diſcordant climates ſeldom intrench much upon each other's territories. The hot winds below, if they aſcend, become cooled by their expanſion, and hence cannot affect the ſnow upon the ſummit; and the cold winds, that ſweep the ſummit, become condenſed as they deſcend, and of temperate warmth, before they reach the fertile plains of Quito.

Correſpondence of the Heat of the Atmoſphere with the Height of the Barometer.

From this principle ſome of the ſudden changes of our atmoſphere from hot to cold, and from dry to moiſt, may likewiſe be accounted for. During the laſt year I frequently obſerved, that when the barometer roſe (the wind continuing in the ſame quarter, *viz.* N.E. or S.W.) the air became many degrees warmer. A ſimilar fact is related from **Musschenbroek**, in Mr. **Kirwan**'s ingenious work on the Temperature of different Latitudes; *viz.* that in winter, when the mercury in the barometer deſcends, the cold increaſes. More accurate obſervations on this ſubject, when the air is ſtationary, or when the wind continues in the ſame quarter, might lead to the diſcovery of the quantity of heat ſqueezed out of the air by a certain preſſure.

The Devaporation of aerial Moiſture.

As heat appears to be the principal cauſe of evaporation, as well as of ſolution, and of fluidity in general, the privation of heat may be eſteemed the principal cauſe of devaporation: for though the air may, by its own power of attraction, or by means of the electricity it may contain, diſſolve and ſuſpend a portion of water, as water diſſolves and ſuſpends a portion of ſalt; yet, by the application of cold, theſe are reſpectively precipitated; and therefore heat may be aſſumed as the immediate cauſe of theſe ſolutions. Add to this, that water boils *in vacuo* with leſs heat; that is, it evaporates *in vacuo* faſter or eaſier than in the open air, and therefore the attractive power of the atmoſphere does not ſeem neceſſary to evaporation.

Now, when the barometer ſinks (from whatever cauſe not yet underſtood this may happen) the lower ſtratum of air becomes expanded by its elaſticity, being releaſed from a part of the ſuper-incumbent preſſure, and, in conſequence of its expanſion, robs the vapour which it contains of its heat; whence that vapour becomes condenſed, and is precipitated in ſhowers, as is viſible in the receiver of an air-pump above mentioned.

There are, however, two other curious circumſtances belonging to the devaporation of water, which have not been perhaps much attended to.

Firſt, that the deduction of a ſmall quantity of heat from a cloud or province of vapour, compared with the quantity of heat which was neceſſary to raiſe that vapour from water, will devaporate the whole. This circumſtance is evident in the operation of common ſteam-engines, in which a ſmall jet of water, whoſe heat is often above 48 degrees, perpetually devaporates the ſteam raiſed by a comparatively very great quantity of heat under the boiler. This difficult problem is explicable from the principles before eſtabliſhed: if a ſmall part of a province of vapour be ſuddenly condenſed, a vacuity takes place, and the contiguous walls of vapour expand themſelves into this vacuity; and thus a large area of vapour, perhaps of many miles in circumference, becomes more or leſs expanded; by this expanſion cold is produced (that is, its capacity of receiving heat is increaſed), and the whole is devaporated.

This very circumſtance exactly takes place in the famous ſteam-engine of Meſſ. WATT and BOULTON; which, from the happy combination of chemical and mechanic power, may juſtly be eſteemed the firſt machine of human invention. In this excellent machine, after the cylinder is filled with ſteam, a communication is opened between this reſervoir of ſteam and a ſmall cell, which is kept cold by ſurrounding water, and free from air by an air-ſyringe adapted to it. What then happens? The corner of the ſteam in the cylinder next to this vacuum (with which it now communicates) ruſhes into it, and the whole ſteam in the cylinder is thus ſuddenly expanded, and inſtantly devaporated: whence the very quick reciprocations of the piſton; and that, though the cylinder itſelf is always kept as hot as boiling water, that is, as hot as the ſteam was previous to its devaporation.

Something very ſimilar to this is often ſeen at the commencement of thunder-ſtorms; a ſmall black cloud at firſt appears, in a few minutes the whole heaven is covered with condenſing vapour, and the accumulation or eſcape of electric matter ſeems to be rather the conſequence than the cauſe of this ſudden and general devaporation.

A ſecond curious circumſtance of aërial devaporation is, that when the particles of aqueous vapour begin to approach each other by the diminution of their heat, they do not generate water exactly in proportion to ſuch diminution of heat; but the condenſation proceeds further, and not only a greater quantity of water is produced, but alſo a quantity of heat is ſet at liberty along with this exceſs of devaporation, and the atmoſphere becomes warmer than before the beginning condenſation. This exceſs of devaporation beyond the cold which produced it, is probably owing to the acquired momentum of the aqueous particles towards each other at the beginning of their condenſation, which carries them ſtill nearer each other; and to the ſmall moleculæ at firſt formed, poſſeſſing a greater attractive power over the uncondenſed vapour in their vicinity, and thus preſſing out more of the latent or combined heat.

Concluſion.

1. When a ſmall portion of air, ſuppoſe a few acres, becomes ſuddenly contracted into a leſs compaſs, either by inci-

dental cold, or by any other cauſe not yet underſtood (as the combination of dephlogiſtic and inflammable gaſes), the air next in vicinity ſuddenly expands itſelf to occupy the vacuity; and by its expanſion produces cold and devaporates, and then becomes compreſſible into leſs ſpace than it occupied before it parted with its vapour. This then gives occaſion to the next circum-ambient portion of air to go through the ſame proceſs, that is, to expand, attract the heat from its vapours, devaporate, and then become compreſſible into leſs ſpace; and thus, from a ſmall and partial contraction or diminution of air, it ſeems poſſible to devaporate a great province.

2. The vapour of a great province of air being thus condenſed, would leave a great vacuity in that part of the atmoſphere, which would be ſupplied by winds ruſhing in on all ſides. Suppoſe this to happen to the north of our climate, a ſouth-weſt wind would be produced here, which is otherwiſe very difficult to underſtand: and if it ſhould ever be in the power of human ingenuity to govern the courſe of the winds, which probably depends on ſome very ſmall cauſes; by always keeping the under currents of air from the S.W. and the upper currents from the N.E. I ſuppoſe the produce and comfort of this part of the world would be doubled at leaſt to its inhabitants, and the diſcovery would thence be of greater utility than any that has yet occurred in the annals of mankind.

November 22, 1787.

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V. *Some Obſervations on the Heat of Wells and Springs in the Iſland of* Jamaica, *and on the Temperature of the Earth below the Surface in different Climates. By* John Hunter, *M. D. F. R. S.; communicated by the Hon.* Henry Cavendiſh, *F. R. S.*

Read December 20, 1787.

TO THE HON. MR. CAVENDISH.

SIR,

THE following obſervations on the heat of ſprings and wells, and their application towards determining the mean temperature of the earth in different climates, were ſuggeſted by you in ſome converſation on that ſubject, previous to my going to Jamaica in 1780. If you think them deſerving the attention of the Royal Society, I muſt beg the favour of you to lay them before that learned Body.

I have the honour to be, &c.

JOHN HUNTER.

Charles-ſtreet,
Dec. 11, 1787.

THE great difference between the temperature of the open air, and that of deep caverns or mines, has long been taken notice of, both as matter of curioſity and ſurprize. After thermometers were brought to a tolerable degree of perfection, and meteorological regiſters were kept with accuracy, it became a problem, to determine what the cauſe was of this difference between the heat of the air, and the heat of the earth; for it was ſoon found, that the temperature of mines and caverns did not depend upon any thing peculiar to them; but that a certain depth under ground, whether in a cave, a mine, or a well, was ſufficient to produce a very ſenſible difference in the heat. In obſervations of this kind, there was perhaps nothing more ſtriking, than that the heat in ſuch caves was nearly the ſame in ſummer and winter; and this even in changeable climates, that admitted of great variation between the extremes of heat in ſummer, and cold in winter. There is an example of this in the cave of the Royal Obſervatory at Paris. The explanations, which have been attempted of this phænomenon, have turned chiefly upon a ſuppoſition, that there was an internal ſource of heat in the earth itſelf, totally independent of the influence of the ſun *. M. DE MAIRAN has beſtowed much labour on this ſubject, and by obſervation and calculation is led to conclude, that of the 1026° of heat (by REAUMUR's ſcale), which he finds to be the heat of ſummer at Paris, 34°,02 only proceed from the ſun, and the remaining 991°,98 from the earth, by emanations of heat from the center †. The proportion therefore of heat derived from this latter ſource is to that of the ſun, as 29,16 to 1. It muſt be evident, that an hypotheſis of this kind, which renders the influence of the ſun of ſmall account, is directly contrary to the general

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* Vid. MARTINE's Eſſays, p. 319.

† Memoir. de l'Acad. des Sciences, An. 1719 et 1765.

experience and conviction of mankind. Without entering, however, into any diſcuſſion of the *data* from whence M. DE MAIRAN draws his concluſions, it will be more ſatisfactory to conſider what the effect of the operation of thoſe laws of heat, with which we are acquainted, would be.

And firſt, it is well known, that heat in all bodies has a tendency to diffuſe itſelf equally through every part of them, till they become of the ſame temperature. Again, bodies of a large maſs are both cooled and heated ſlowly. Beſides the maſs of matter, there are two other conſiderations of much importance in the ſlow or quick tranſmiſſion of heat through bodies; theſe are their different conducting powers, and their being in a ſtate of ſolidity or fluidity. The conducting powers of heat are well known to be very various in different bodies; nor are they hitherto reducible to any law, depending either upon the denſity, or chemical properties of matter. Metals of all kinds are good conductors of heat, while glaſs, an heavy, ſolid, homogeneous body, is an extremely bad conductor, even when a metallic calx enters largely into its compoſition, as in flint-glaſs. A ſtate of fluidity greatly promotes the diffuſion of heat; for a body in a fluid ſtate, by the particles moving readily among each other from their different denſities or other cauſes, mixes the warm and cold parts together, which occaſions a quick communication of heat. To apply theſe obſervations to the preſent ſubject; the ſurface of the earth being expoſed to the great heats of ſummer, and the colds of winter, or more properly the low degree of heat of winter, will receive a larger proportion of heat in the former ſeaſon, and a ſmaller in the latter; and being further of a large maſs, and of a porous and ſpongy ſubſtance, and therefore not quickly ſenſible to ſmall variations of heat, it will become of a mean temperature at a certain depth, between the heat of ſummer, and the cold of winter, provided it contain no internal ſource of heat within itſelf. This concluſion is ſtrictly agreeable to the experiments and obſervations hitherto made, in heating and cooling bodies, or in mixing portions of matter of the ſame kind of different temperatures*. Water, though in a large maſs, follows in ſome degree the heat and cold of our ſummer and winter, from the mobility of its parts occaſioning a more ſpeedy diffuſion of heat. Air is quickly ſuſceptible of heat, and from the expanſions produced in it, and conſequent motions in the whole maſs, the temperature is ſoon rendered uniform.

The changes in the heat of the air are what we have meaſured, and we are to be underſtood to ſpeak of them, when we talk of the temperature of ſummer and of winter. It may be aſked then, is the heat of the ſun firſt communicated to the air, and thereby to the earth? No, the air is ſuſceptible of a very ſmall degree of heat from the rays of the ſun paſſing through it; for it is well known, they produce no heat in a tranſparent medium, and conſequently, that the air is only ſo far heated as it differs from a medium that is perfectly tranſparent. The heat produced by the rays of the ſun bears a proportion to their number, their duration, and their falling more or leſs perpendicularly; and it takes place at the points where they ſtrike an opaque and non-reflecting ſurface. The ſurface of the earth may therefore be conſidered as the place, from whence the heat proceeds, which is communicated to the air above, and the earth below. That this is really the caſe is evident from the ſuperior degree of heat, produced by the action of the rays of the ſun upon an opaque body, which will often be heated to 150° (FAHRENHEIT), while the temperature of the air is not above 90° †. It may ſeem, therefore, that to meaſure the heat communicated

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* Vid. DE LUC Modifications de l'Atmoſphere, Vol. I. p. 285.

† MARTINE's Eſſays, p. 309.

to the earth, it ſhould be done at the ſurface, where the action of the rays immediately takes place. But though the heat be produced at the ſurface, it is communicated freely to the air as well as the earth; and though the apparent intenſity of heat be greater in the earth, from the rays of light acting for a longer time upon the ſame parts of matter, yet there is little doubt that much the greater part is carried off by the air, which as it is heated flies off, and allows a freſh portion of cold air to come in contact with the heated ſurface. But ſtill it is immaterial, whether the heat of the ſun be excited more in the earth or in the air; for whichever has the larger proportion will in the end communicate a part to the other, and ſo reſtore the balance. The ſame obſervation applies to ſuch cauſes of cold as may operate at the ſurface of the earth, as evaporation, and that taken notice of by Mr. WILSON *. The air, therefore, near the ſurface of the earth will ſhew by a thermometer in the ſhade nearly, if not exactly, the ſame degree of heat that the ſun communicates to our terreſtrial globe; and if a mean of the heats thus ſhewn be taken for the year round, and we penetrate into the earth to that depth, that it is no longer affected either by the daily, monthly, or annual variations of heat, the temperature at ſuch depth ſhould be equal to the annual mean above mentioned. To aſcertain this with the utmoſt preciſion, it muſt be obvious, that numerous obſervations ſhould be made every day, correſponding to the frequent changes of temperature, which are known to happen in the courſe of the twenty-four hours in all climates; and upon theſe a daily mean ſhould be taken, and the annual mean deduced therefrom. This has not yet been done, but where we have obſervations from which a mean temperature can be deduced with any degree of certainty, it will be found not to differ greatly from

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* Vid. Phil. Tranſ. Vol. LXX. p. 451. and Vol. LXXI. p. 386.

the heat of deep caves, or wells in the ſame climate. If further experience and obſervation ſhould confirm the above opinions, it will be attended with this advantage, that we ſhall be poſſeſſed of an eaſy and ready method of aſcertaining the mean temperature of any climate; which, with a few obſervations of the extremes of heat and cold at particular ſeaſons, will teach us as much of the country, with regard to heat and cold, as the meteorological obſervations of ſeveral years.

For obtaining the temperature of the earth the beſt obſervations are probably to be collected from wells of a conſiderable depth, and in which there is not much water. Springs iſſuing from the earth, although indicating the temperature of the ground from whence they proceed, are not ſo much to be depended upon as wells; for the courſe of the ſpring may be derived from high grounds in the neighbourhood, and it will thence be colder; it may run ſo near the ſurface as to be liable to variations of heat and cold from ſummer and winter; or it may be expoſed to local cauſes of heat in the bowels of the earth. Wells ſeem alſo better than deep caverns, for the apertures to ſuch are often large, and may admit enough of the external air to occaſion ſome change in their temperature. Wells are not, however, to be met with in all places, and in that caſe we muſt remain ſatisfied with the temperature of the ſprings.

The following obſervations were made in the Iſland of Jamaica, where there are flat lands in many parts towards the coaſt, but all the interior part of the country is mountainous. The heat is greateſt in the low lands, and decreaſes as you aſcend the mountains. The town of Kingſton is ſupplied with water from wells. The ground on which it ſtands riſes with a gentle aſcent as you recede from the ſea. In the low part of the town the wells are but a few feet deep, and many of them brackiſh. The heat of the water in ſome of them I have found as high as 82°; but they were evidently too near the ſurface not to be affected by the heat of the ſeaſons. As you aſcend, the wells are deeper, and the temperature is nearly 80° in all of them. What variations there are, come within one degree, that is, half a degree leſs than 80°, or half a degree more. They are of different depths, and ſome not leſs than 100 feet; though, after they are of half that depth, the temperature is nearly uniform. At the *Governor's Pen*, which is alſo in the low part of the country, a well, which is above 60 feet deep, is 79½°. There is a well at Half-way-Tree, 243 feet deep, which is 79°. Half-way-Tree is two miles from Kingſton, with a very gentle aſcent. Near Rock-Fort is a ſpring, immediately at the foot of the long mountain, which throws out a great body of water; the heat of it is 79°. All the places mentioned are but very little above the level of the ſea, probably not more than the depth of the wells at the reſpective places; for near Kingſton there are ſprings that appear juſt below the water-mark of the ſea, and thoſe that ſupply the wells are probably upon the ſame level.

The temperature of the air at Kingſton admits but of ſmall variation. The thermometer, at the hotteſt time of the day, and during the hotteſt ſeaſon of the year, ranges from 85° to 90°; in the cooleſt ſeaſon, and obſerved about ſun-riſe, which is the coldeſt time in the twenty-four hours, it ranges from 70° to 77°. I have ſeen it once as low as 69°, and two different times as high as 91°. The annual mean temperature cannot, therefore, either much exceed, or fall much ſhort of, 80°, as indicated by the wells.

The following ſprings were examined with much accuracy by the Hon. Mr. SEWELL, Attorney General of the Iſland.

Ayſcough's ſpring, on the road from Spaniſh Town to Puſey's, in St. John's pariſh, 75°.

Puſey's ſpring, ſtill higher in the mountains, 72°¾.

A ſpring near the baracks at Points Hill in St. John's pariſh, 70°.

The thermometer in the ſhade at Puſey's, during part of the month of June, was found to range from 69°½ to 79°½. It was obſerved both late at night, and early in the morning before ſun-riſe.

The ſpring in Brailsford Valley, about ten miles above Spaniſh Town, is 75°. The ſpring at Stoney Hill is 71°. Theſe were examined by Mr. HOME.

Mr. WALLEN's houſe, at Cold Spring, ſtands the higheſt of any in the iſland. By a meaſurement, ſaid to have been made by Mr. M^c FARLANE, it is reported to be 1400 yards above the level of the ſea. On the road to it, and about a mile below Mr. WALLEN's houſe, there is a ſpring that iſſues from the ſide of the hill, of the temperature of 65°. Cold Spring, which gives a name to the place, is about fifty feet below the houſe, and the heat of it is 61°¾. The thermometer in the ſhade at Mr. WALLEN's houſe, for ſome days in the month of April, ranged from 57° to 67°. It may be remarked, that the higher the ſprings the colder they are; and, as far as a conjecture can be formed from ſo few obſervations, they would appear not to differ much from the mean temperature of their reſpective places *.

It will not be out of place to add ſome obſervations made in England, relative to the ſame ſubject. The wells in and

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* The thermometers made uſe of were all made by Mr. RAMSDEN.

about London are either of no great depth, or are full of water, which are both conſiderable objections to their giving a mean temperature. The want of depth will make them ſubject to the variations of the ſeaſons; and a large quantity of water, even in a deep well, will take the temperature of the air more or leſs: for any change of temperature communicated at the ſurface will, from the fluidity of the water, be readily diffuſed through the whole. I ſuſpect it is owing to this cauſe, that the wells in the neighbourhood of Brighthelmſtone vary from 50° to 52°, for thoſe were the higheſt that had moſt water in them. My obſervations were made in ſummer. Theſe wells are of various depths, from 15 to 150 feet. That which I always found the coldeſt is not more than 22 feet deep; I never found its heat greater than 50°. It is near the beach, and is a tide well, that is, the water in it riſes and falls, and in ſo doing does not correſpond exactly with the tides, but follows them with an interval of about three hours. At the loweſt there is not more than a foot of water in it; and it may be conſidered as a ſubterraneous ſpring running through the bottom of the well. There are in fact numerous ſprings that break out upon the ſand, a few feet above the low-water mark, which are doubtleſs the ſame that ſupply the wells. As we are not acquainted with any cauſe that produces cold in the bowels of the earth, we muſt neceſſarily in every climate, conſider the loweſt degree of heat as approaching neareſt to the mean temperature; and therefore we cannot conclude the mean temperature at Brighthelmſtone to be more than 50°. The mean temperature of London is computed about 52°[*]; but Brighthelmſtone is nearly fifty miles farther ſouth than London, and is immediately upon the ſea, and muſt therefore be at leaſt as warm as London. It is evident, that the obſervations from which the mean is taken, muſt generally contain more of the extremes of heat than of cold, as the former happen in the day-time, and the latter in the night, in conſequence of which they will often eſcape notice. There is a table conſtructed by Dr. HEBERDEN *, expreſſing the heat in London for every month in the year, from a mean of ten years beginning with 1763, and ending with 1772. The mean temperature is given both at 8 A.M. and 2 P.M. There is further in the table, a column of the mean of the greateſt monthly colds in the night, obſerved during the ſame ten years by Lord CHARLES CAVENDISH, in Marlborough-ſtreet. There will not probably be any great error in conſidering the heat obſerved at 2 P.M. as the greateſt daily heat; and taking a mean btween the greateſt heats of the day, and greateſt colds of the night, they give 49°,196 for an annual mean, which is much lower than is commonly ſuppoſed. At the houſe of GEORGE GLENNY, Eſq. near Bromley, there is a well ſeventy-five feet deep, which I found in November 49°½. M. DE MAIRAN has given a table of the greateſt heats and greateſt colds obſerved at Paris for fifty-ſix years, beginning from 1701; and a mean of them is 10° above freezing, or 1010°, of REAUMUR's ſcale †. The temperature of the cave of the Obſervatory where thoſe obſervations were made, is 10°¼ above freezing, by the ſame ſcale of REAUMUR. There appears not therefore any neceſſity for an internal heat; on the contrary, it is matter of demonſtration, that were there any ſource of heat in the earth which was not equally in the air, the heat of the interior parts ought to be higher

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[*] KIRWAN's Temperature of different Latitudes, p. 73.

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* The Table alluded to follows this Paper.

† Mem. de l'Acad. des Sciences, An. 1765, p. 202.

higher than a mean: and did the *central heat* bear as high a proportion to that of the ſun as M. de Mairan alledges, the heat of the earth itſelf ought to be a great deal above the mean temperature of the air, which from obſervation there is no ground for believing. It is eaſy to ſee the ſource of M. de Mairan's error; he has founded his calculations upon the ſcale of Reaumur, and conſiders the degrees of his thermometer as marking the real proportions, and abſolute quantity of heat *. It is a matter that cannot be denied, that we know nothing of the abſolute quantities of heat; and that the degrees of our thermometers are only to be conſidered as a few of the middle links of a chain, the length of which we are totally ignorant of, and therefore in no condition to compare its proportional parts. It deſerves, however, to be remarked, that obſervations of a late date have ſhewn, that the notions of cold upon which Reaumur's ſcale was conſtructed, and upon which M. de Mairan's calculations are founded, are imaginary and without foundation †.

Hot ſprings and volcanos may be produced as proofs of the exiſtence of an internal ſource of heat in the earth; but their operation appears to be limited to a very ſmall extent, and ſcarcely deſerving of notice in the preſent diſcuſſion. It is no uncommon thing to find ſprings of the uſual temperature cloſe by hot ſprings; and no volcano, with which we are yet acquainted, appears to have raiſed the temperature of the country immediately adjoining to it.

The ſea admits of change of temperature more quickly than the earth, particularly near the ſhore. The mean heat of the ſea at Brighthelmſtone, during the months of July, Auguſt, September, and October, was as follows:

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* Vid. Memoir. de l'Acad. des Sciences, An. 1765, p. 143.

† Vid. Phil. Tranſ. Vol. LXXIII. p. *303. 303. and 329.

July	$63°\frac{1}{7}$
Auguſt	$63°\frac{1}{2}$
September	58°
October	53°

The obſervations were made with a view to aſcertain the temperature of the ſea as a bath, and therefore the heat was taken about nine in the morning, and near the ſhore, the uſual time and place of bathing. The water gets hotter towards three o'clock in the afternoon, ſo that it not only follows the monthly, but even the daily changes of the temperature of the air. In the four months juſt mentioned, the extremes of heat and cold are conſiderable: I have ſeen it as hot as 71°, and as cold as 49°. In the month of Auguſt laſt, Sir HENRY ENGLEFIELD examined the heat of the ſea at the ſame time that I did, and we both found it 71°: it was about 4 P.M. of a very hot day. I may be allowed to remark, that ſea-bathing is a very different thing at different ſeaſons of the year, and requires an acquaintance with the variations of the temperature, to adapt it to particular caſes.

It were to be wiſhed, that the heat of wells and ſprings were examined at different ſeaſons of the year, in order to aſcertain the effect of ſummer and of winter upon them. The wells at New York are from 32 to 40 feet in depth, and Dr. NOOTH found them to have an annual variation of two degrees from 54° to 56°. There are few countries, in which the annual range of the thermometer is greater than at New York, and the neighbouring parts of America. In the ſummer it is often as high as 96°, and in winter it has been obſerved ſeveral degrees below the zero of FAHRENHEIT's ſcale.

We may, I think, from all the obſervations we are yet in poſſeſſion of, conclude, that there is at preſent no ſource of heat in the earth, capable of affecting the temperature of a country, which is not derived from the ſun; and that the earth, whatever changes of temperature it may be conjectured to have undergone in former periods, is now reduced to a mean of the heat produced by the ſun in different ſeaſons, and in different climates.

VI. *A Table of the mean Heat of every Month for Ten Years in* London, *from* 1763 *to* 1772 *incluſively. By* William Heberden, *M. D. F. R. S. and A. S.*

Read January 31, 1788.

		At 8 A.M.	At 2 P.M.	Mean.	Night.
12	January	35°	39°	37°	34.7°
10	February	38	43	40.5	36.6
9	March	39	45	42	37.1
7	April	44	52	48	41.3
5	May	51	59	55	46.4
3	June	57	65	61	52.4
2	July	59	68	63.5	55.6
1	Auguſt	60	68	64	55.1
4	September	55	63	59	51.7
6	October	48	55	51.5	45.5
8	November	43	48	45.5	40
11	December	39	42	40.5	37.3

EXPLANATION OF THE TABLE.

The firſt column of figures denotes the order of the months according to their degrees of heat, beginning with Auguſt, in which the heat is greateſt.

The ſecond, and third, are the heats marked at the hour expreſſed at the top of each column, and the fourth is the mean between theſe two.

The laſt column is the mean of the greateſt cold at night, obſerved in Marlborough-ſtreet for twenty years, by the late Right Honourable Lord **CHARLES CAVENDISH**.

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VII. *On Centripetal Forces.* *By* Edward Waring, *M. D. F. R. S. Profeſſor of Mathematics at* Cambridge.

Read January 10, 1788.

PROP. I.

1. LET a curve P*p*N (Tab. II. fig. 1.), of which the perpendiculars to the two neareſt points P and *p* of the curve are PO and *p*O, and conſequently O the center of a circle, which has the ſame curvature as the given curve in the point P; draw PY and *ly* tangents to the curve in the points P and *p*; from S draw S*y* and S*h*Y reſpectively perpendiculars to the tangents *ly* and PY; and let S*h*Y cut the tangent *ly* in *h*; then will ultimately *h*Y ($-\dot{P}$) be the decrement of the perpendicular SY = P; and the triangles *lh*Y and PO*p* be ſimilar: for the angles PO*p* and *hl*Y are equal, and the angles *l*Y*h* and OP*p* right ones; therefore PO : P*p* :: *l*Y ultimately = PY : Y*h* decrement of the perpendicular, whence $Pp = \frac{Yh \times PO}{lY} = \frac{Yh \times PO}{PY}$.

1.2. Fig. 2. and 1. The force in the direction PS is as the ultimate ratio of $2 \times QR$ (the ſpace through which a body is drawn from the direction of its motion in the tangent in a given time towards the center of force); but ultimately $2QR = \frac{2QP^2}{PV}$, where QP is as the ſpace deſcribed in a given time, and conſequently as the velocity (V) of the body at the given point P, and PV the chord of curvature in the direction SP.

1.3. The increment (Pp) of the ſpace divided by the velocity V is ultimately as the increment of the time, and $=$ the increment of the velocity ($\dot{V}$) divided by the force $\frac{2V^2}{PV} \times \frac{PY}{SP}$ in the direction of the tangent, that is, $\frac{Pp}{V} = \frac{\dot{V} \times PV \times SP}{2V^2 \times PY}$; for Pp ſubſtitute $\frac{Yb \times PO}{PY}$, and there reſults $\frac{-\dot{P} \times PO}{PY \times V} = \frac{\dot{V} \times PV \times SP}{2V^2 \times PY}$; and conſequently $\frac{-2\dot{P} \times PO}{SP \times PV} = \frac{\dot{V}}{V}$; but $\frac{SP \times PV}{2PO} = SY = P$, whence $\frac{-\dot{P}}{P} = \frac{\dot{V}}{V}$, and $V = \frac{a}{P}$, where a is an invariable quantity.

Cor. Since $V \times P$, that is, SY the perpendicular multiplied into the velocity (which is ultimately as Pp the ſpace deſcribed in a given time) is ultimately as the area deſcribed round the center S in a given time; but this rectangle $= a$, a given quantity; therefore the area, deſcribed round the center of force S in a given time, will be a given quantity, and thence in unequal times will be proportional to the times.

1.4. The ſagitta QR is ultimately as the force, when the time is given; and when the time is not given, it will be as the force into the ſquare of the time; from which expreſſion, by ſubſtituting for QR and the time their values, may be deduced ſeveral others.

Sir ISAAC NEWTON has demonſtrated this propoſition with the greateſt ſimplicity; and this is given to ſhew, that the ſame propoſition may be deduced from different principles.

PROP. II.

1. Fig. 3. Given the relation between SP′ the diſtance from a point S, and SY′ a perpendicular from the point S to P′Y, a line touching a curve in the point P′; to find the relation

between Sp' and Sy' (a perpendicular from the point S to $p'y$, a line touching the curve in the point p'); in which two curves $PP'L$ and $pp'l$, the forces and velocities at any equal diſtances SP and Sp are equal, and conſequently the perpendiculars SY and Sy, at the above-mentioned equal diſtances SP and Sp are to each other in a given ratio $N : n$.

In the equation expreſſing the relation between SP' and SY' for SP' and SY' write reſpectively Sp and $\frac{Sy' \times N}{n}$, and there reſults the equation ſought: for the diſtances SP and Sp' being equal, the perpendiculars SY' and Sy' are as $N : n$.

Ex. 1. Let S be the focus of a conic ſection, then will $\frac{1}{4} C^2 \times \frac{D}{T \pm D} = SY^2 = P^2$, where T and C denote its tranſverſe and conjugate axes, and D the diſtance SP; for P write $\frac{N}{n} \times p$, and there reſults the equation $\frac{1}{4} C^2 \times \frac{D}{T \pm D} = \frac{N^2}{n^2} \times p^2$, which is an equation to a conic ſection of the ſame name (*viz.* ellipſe, parabola, or hyperbola) as the given curve, of which the tranſverſe axis is T, and conjugate $= \frac{C \times n}{N}$, and perpendicular from the focus to the tangent $= p$. If T and C are infinite, and conſequently the curve a parabola, and the equation $\frac{1}{4} L \times D = P^2$, then will the *latus rectum* of the reſulting equation be $\frac{L \times n^2}{N^2}$.

Ex. 2. Let S be the center of the logarithmic ſpiral, then will the equation be $a \times SP = a \times D = SY = P$, and conſequently the reſulting equation $a \times D = \frac{N}{n} \times p$, whence $\frac{an}{N} \times D = p$ an equation to a logarithmic ſpiral having the ſame center.

Ex. 3. Let T and C be the ſemi-conjugate axes of a conic ſection, and S its center; then will the equation expreſſing the relation between the diſtance D and perpendicular P be $D^2 \pm \frac{T^2C^2}{P^2} = T^2 \pm C^2$; for P write as before $\frac{Np}{n}$, and there reſults the equation $D^2 \pm \frac{n^2T^2C^2}{N^2p^2} = T^2 \pm C^2$, an equation to a conic ſection of the ſame name, of which the tranſverſe and conjugate diameters are reſpectively two roots (x) of the equation $x^2 \pm \frac{n^2T^2C^2}{N^2x^2} = T^2 \pm C^2$, becauſe in this caſe $p = D$.

The ſum or difference of the ſquares of the tranſverſe and conjugate diameters, in all the reſulting equations, will be the ſame.

Cor. In every equal diſtance, the chord of curvature paſſing through the center of force is the ſame; for the forces in that direction, and the velocities at every equal altitude are the ſame.

PROP. III.

1. Fig. 4. and 3. Given an equation $A = o$, expreſſing the relation between the abſciſs $SM = x$ and ordinate $MP = y$; to find the equation expreſſing the relation between $SP = \sqrt{x^2 + y^2}$ and $SY = P$, the perpendicular from S on the tangent PY. From the equation $A = o$ find $\dot{x} = B\dot{y}$, which ſubſtitute for $\dot{x}$ in the equation $(\dot{x}^2 + \dot{y}^2)^{\frac{1}{2}} \times P = \dot{x}y \pm x\dot{y}$ deduced from the ſimilar triangles P*lo*, MTP, and STY, where $lo = \dot{x}$ and $Po = \dot{y}$; let the reſulting equation be $C = o$; reduce the three equations $A = o$, $C = o$, and $x^2 + y^2 = SP^2 = D^2$ into one, ſo that the unknown quantities x and y may be exterminated, and there reſults an equation expreſſing the relation between D and P.

Cor. Hence from the equation expreſſing the relation between x and y, the abſciſs and ordinate of a curve, can be deduced an equation expreſſing the relation between the diſtance SP and perpendicular SY; and from the equation expreſſing the relation between the diſtance SP and SY can be deduced an equation expreſſing the relation between the diſtance S*p* and perpendicular S*y* from the point S to the tangent *py* of a curve, whoſe force and velocity at every equal diſtance is the ſame as in the given curve, but the direction different.

2. Given an equation $K=o$ expreſſing the relation between $SP=D$ and $SY=P$; to find an equation expreſſing the relation between $SM=x$ and $PM=y$, the abſciſs and ordinate of the ſame curve.

In the given equation $K=o$ for D and P write reſpectively $\sqrt{x^2+y^2}$ and $\frac{y\dot{x}\pm x\dot{y}}{\sqrt{\dot{y}^2+\dot{x}^2}}$, and there reſults a fluxional equation $L=o$ of the firſt order, of which the fluent expreſſes the general relation between x and y.

Cor. If in the given equation for P be wrote nP', there reſults the equation $K'=o$, which expreſſes the relation between the perpendicular $Sy=P'$ and diſtance $Sp=D'$ of every curve, which at equal diſtances has the ſame velocity and force tending to S; reduce the equations $K'=o$, $D=\sqrt{x^2+y^2}$ and $nP'=\frac{y\dot{x}\pm x\dot{y}}{\sqrt{\dot{x}^2+\dot{y}^2}}$ into one, ſo that D and P' may be exterminated, and there will reſult the ſame fluxional equation of the firſt order, expreſſing the relation between x, y, and their fluxions, whatever may be the value of n. The general fluent of this fluxional equation contains the relation between the abſciſs and ordinates of all curves, which have the ſame force and velocity at the ſame diſtance as the force and velocity in the given curve.

PROP.

PROP. IV.

1. Let a body move in a given curve PH (fig. 5.), of which the velocity (v) at any point P is given: and let the forces f'', f''', &c. tending to all the given centers S″, S‴, &c. (except two S and S′) be given; to find the forces f and f' tending to the two points S and S′.

Draw a line PO perpendicular to the tangent yPy'; and from the given centers S, S′, S″, &c. draw lines Sl and Sy, S′l' and S′y', S″l'' and S″y'', &c. perpendicular to the lines PO and yPy', &c.; then will $\frac{v^2}{PO} = f \times \frac{Pl}{PS} \pm f' \times \frac{Pl'}{PS'} \pm f'' \times \frac{Pl''}{PS''} \pm$ &c. (where PO is the radius of the circle having the ſame curvature as the curve in the point P), and $\frac{-v\dot{v}}{\dot{A}} = f \times \frac{Py}{PS} \pm f \times \frac{Py'}{PS} \pm$ &c. (where A denotes the arc of the curve PH); from the *data* may be deduced all the quantities contained in the above mentioned two equations, except f and f'; and conſequently from the two given ſimple equations be deduced the forces ſought f and f'.

2. Let the velocity of the body moving in the given curve be ſuppoſed always uniform; then $f \times \frac{Py}{PS} \pm f' \times \frac{Py'}{PS'} \pm f'' \times \frac{Py'}{PS''} \pm$ &c. $= a$.

Ex. Let,the curve HPl be an ellipſe, and the two foci S and S′ the centers of forces; then will $f \times \frac{Py}{SP} = f' \times \frac{Py'}{SP'}$, but the angle $SPy = S'Py'$, and conſequently $\frac{Py}{SP} = \frac{Py'}{S'P}$ and $f = f'$; but ſince $\frac{v^2}{PO} = f \times \frac{Sy}{SP} + f \times \frac{Sy'}{SP} = 2f \times \frac{Sy}{SP}$, and $v = a$, then will $f = \frac{a^2 \times SP}{2Sy \times PO}$ be the force tending to each focus.

In theſe and the ſubſequent caſes the lines Py, Py', Py'', &c. are to be taken negatively or affirmatively, as they are ſituated on the ſame or different ſides of P; and in the ſame manner the lines Pl, Pl', Pl'', &c. are to be taken negatively or affirmatively as they are ſituated on the ſame or different ſides of the tangent yPy', &c.

3. Let the centers M, M′, M″, M‴, &c. of forces be points not ſituated in the plane of the given curve HPI, &c. and the forces f''', f'''', &c. tending to each of the centers M‴, M⁗, &c. (except three M, M′, and M″) be given; to find the forces f, f', and f'' tending to thoſe three points M, M′, and M″.

Draw MS, M′S′, M″S″, &c. perpendicular to the plane HPI, &c. from the above-mentioned points, and aſſume the equation $f \times \frac{MS}{\sqrt{MS^2+SP^2}} \pm f' \times \frac{M'S'}{\sqrt{M'S'^2+S'P^2}} \pm f'' \times \frac{M''S''}{\sqrt{M''S''^2+S''P^2}} \pm f''' \times \frac{M'''S'''}{\sqrt{M'''S'''^2+S'''P^2}} + \&c. = o$, and the two preceding equations $\frac{v^2}{PO} = f \times \frac{Pl}{PM} \pm f' \times \frac{Pl'}{PM'} \pm f'' \times \frac{Pl''}{PM''} \pm$ &c. and $\frac{-v\dot{v}}{\dot{A}} = f \times \frac{Py}{PM} \pm f' \times \frac{Py'}{PM'} \pm f'' \times \frac{Py''}{PM''} \pm$ &c.; from the *data* may be found all the quantities f''', f'''', &c.; and conſequently from the above mentioned equations may be deduced the forces f, f', and f''.

4. Let the body move in different planes, that is, in a curve of double curvature at the ſame points; draw PR a tangent to the curve at the point P, and PQ an arc of the curve of double curvature; draw alſo two planes PRV and PRT, cutting one another in the line PR; from the point Q let fall QV and QT perpendicular to thoſe planes reſpectively, and from the points V and T draw Vv and Tt reſpectively perpendicular to the line PR; let v be the velocity of a body moving in the given

curve at the point P, and aſſume $\frac{PQ^2}{Vv} = 2C$ and $\frac{PQ^2}{Tt} = 2C'$ reſpectively; from the given centers of forces M, M′, M″, M‴, &c. draw MS, MS′, M″S″, M‴S‴, &c.; M*s*, M′*s*′, M″*s*″, M‴*s*‴, &c. reſpectively perpendicular to the two planes RPV and RPT; and PL and P*l* perpendicular to the line PR in the ſame two planes RPV and RPT; and alſo SP, S′P, S″P, S‴P, &c.; *s*P, *s*′P, *s*″P, &c.: from the points S, S′, S″, S‴, &c. *s*, *s*′, *s*″, &c. draw the lines SH, S′H′, S″H″, S‴H‴, &c. *sb*, *s*′*b*′, *s*″*b*″, *s*‴*b*‴, &c. reſpectively perpendicular to the lines PL and P*l*; and SK, S′K′, S″K″, S‴K‴, &c. *sk*, *s*′*k*′, *s*″*k*″, *s*‴*k*‴, &c. perpendicular to the line RP; and let the forces f''', f'''', &c. tending to all the points M‴, M⁗, &c. (except three, M, M′, and M″) be given; then from the three given equations $\frac{v^2}{C} = \frac{PH}{MP} \times f \pm \frac{PH'}{M'P} \times f' \pm \frac{PH''}{M''P} \times f'' \pm$ &c. and $\frac{v^2}{C'} = \frac{Pb}{MP} \times f \pm \frac{Pb'}{M'P} \times f' \pm \frac{Pb''}{M''P} \times f'' \pm$ &c. and $\frac{-v\dot{v}}{\dot{A}} = \frac{PK}{MP} \times f \pm \frac{PK'}{M'P} \times f' \pm \frac{PK''}{M''P} \times f'' \pm \frac{PK'''}{M'\,P} \times f''' \pm$ &c. $= \frac{Pk}{MP} \times f \pm \frac{Pk'}{M'P} \times f \pm \frac{Pk''}{M''P} \times f''$ ± &c. which contain only three unknown quantities, can be deduced the forces f, f', and f'', required, tending to the points M, M′, and M″.

PROP. V.

Let a body acted on by forces tending to any given points S, S′, S″, &c. move in a given curve, to find its velocity in any point of the curve.

Find the fluent of the fluxion $(f \times \frac{Py}{PS} \pm f' \times \frac{Py'}{PS'} \pm f'' \times \frac{Py''}{PS''} \pm f''' \times \frac{Py'''}{PS'''} \pm$ &c.) $\dot{A} = f\dot{D} \pm f'\dot{D}' \pm f''\dot{D}'' \pm$ &c. $= -v\dot{v}$ when the forces are all contained in the ſame plane; or the fluent of

$(f \times \frac{Py}{PM} \pm f' \times \frac{Py'}{PM'} \pm f'' \times \frac{Py''}{PM''} \pm \&c.) \times \dot{A}$ (when contained in different planes) $= f \times \dot{P}M \pm f' \times \dot{P}M' \pm f'' \times \dot{P}M'' \pm \&c. = f \times \dot{D} \pm f' \times \dot{D}' \pm f'' \times \dot{D}'' \pm \&c.$; but ſince f, f', f'', &c. are given functions of the quantities D, D′, D″, &c. the fluents of $f \times \dot{D}$, $f' \times \dot{D}'$, $f'' \times \dot{D}''$, &c. can be found; which, when properly corrected will be as $\frac{v^2}{2} = \frac{1}{2}$ the ſquare of the velocity in any point P. A denotes the arc of the curve, and D, D′, D″, &c. the reſpective diſtances of the body from the centers of forces.

Cor. The increment of the time of deſcribing any arc of the above-mentioned curve will be as the increment of the arc $= \dot{A}$ divided by the velocity found above, and conſequently the time itſelf will be as the fluent of it properly corrected.

PROP. VI.

1. Let a body move in any curve, and be acted on by forces tending to any given points, S, S′, S″, S‴, &c.; all of which, except the force f tending to the point S, let be given, to find f the force tending to S.

Let Sy, $S'y'$, $S''y''$, &c. be perpendicular to the tangent Py of the curve at the point P; reſolve the forces f, f', f'', f''', &c. tending to S, S′, S″, S‴, &c. reſpectively into two forces, of which one acts perpendicular to Py, the other, Sl, $S'l'$, $S''l''$, &c. perpendicular to PO, which is perpendicular to Py; let PO be radius of the circle of the ſame curvature as the curve, and v the velocity of the body at the point P; then will $\frac{v^2}{PO} = f \times \frac{Sy}{SP} \pm f' \times \frac{S'y'}{S'P} \pm f'' \times \frac{S''y''}{S''P} \pm f''' \times \frac{S'''P}{S'''y'''} \pm \&c.$ and $-\frac{v\dot{v}}{\dot{A}} = f \times \frac{Py}{SP} \pm f' \times \frac{Py'}{S'P} \pm f'' \times \frac{Py''}{S''P} \pm f''' \times \frac{Py'''}{S'''P} \pm \&c.$: for $\frac{Sy \times PO}{SP} =$

$\frac{1}{2}$ chord of the circle of curvature, which paſſes through S, write C; and for $PO \times (\pm f' \times \frac{S'y'}{S'P} \pm f'' \times \frac{S''y''}{S''P} \pm \&c.)$ ſubſtitute H, and for $\frac{Py}{SP}$ write B; and for $\pm f' \times \frac{Py'}{S'P} \pm f'' \times \frac{Py''}{S''P} + \&c.$ ſubſtitute D, and the two preceding equations become $v^2 = f \times C + H$ and $-v\dot{v} = (Bf + D)\dot{A}$, where $\dot{A}$ denotes as before the increment of the arc of the curve: from the firſt equation $v\dot{v} = \frac{f \times \dot{C} + C\dot{f} + \dot{H}}{2} = -(Bf\dot{A} + D\dot{A})$ and conſequently $C\dot{f} + (\dot{C} + 2B\dot{A})f + \dot{H} + 2D\dot{A} = o$, from which fluxional equation may be deduced the force f tending to the center (S) $= -C^{-1} \times e^{-\int \frac{2B\dot{A}}{C}} \times \int (\dot{H} + 2D\dot{A}) \times e^{\int \frac{2B\dot{A}}{C}}$, where e is the number, whoſe hyper. log. = 1.

Cor. Fig. 1. Let f', f'', f''', &c. be each $= o$, then will $D = o$, $H = o$, and conſequently $\int (2\dot{D}A + \dot{H}) \times e^{\int \frac{2B\dot{A}}{C}} =$ conſt $= a$, and $f = -a \times C^{-1} \times e^{-\int \frac{2B\dot{A}}{C} = \frac{2B \times S\dot{y} \times PO}{C \times Py} = \frac{2S\dot{y}}{Sy}} = -aC^{-1} \times e^{-\int \frac{2S\dot{y}}{Sy}}$; whence $f = \frac{-a}{Sy^2 \times C}$ as is generally known, where a denotes an invariable quantity.

Cor. The force f being found, the ſquare of the velocity may be deduced from the equation $v^2 = f \times C + H$, and the time from the fluent of the fluxion $\frac{\dot{A}}{v} = \frac{\dot{A}}{\sqrt{f \times C + H}}$.

2. Let the body move in a curve of double curvature, and let the forces f'', f''', &c. tending to all the points M″, M‴, &c. (except two, M and M′) be given; to find the forces tending to the points M and M′.

Aſſume the three equations before given in Prop. 4. *viz.* $\frac{v^2}{\mathrm{C}} = \frac{\mathrm{PH}}{\mathrm{MP}} \times f \pm \frac{\mathrm{PH}'}{\mathrm{M'P}} \times f' \pm \frac{\mathrm{PH}''}{\mathrm{M''P}} \times f'' \pm$ &c. $\frac{v^2}{\mathrm{C}'} = \frac{\mathrm{P}b}{\mathrm{MP}} \times f \pm \frac{\mathrm{P}b'}{\mathrm{M'P}} \times f \pm \frac{\mathrm{P}b''}{\mathrm{M''P}} \times f'' \pm$ &c. and $-v\dot{v} = \left(\frac{\mathrm{PK}}{\mathrm{MP}} \times f \pm \frac{\mathrm{PK}'}{\mathrm{M'P}} \times f' \pm \frac{\mathrm{PK}''}{\mathrm{M''P}} \times f'' \pm \text{\&c.} = \frac{\mathrm{P}k}{\mathrm{MP}} \times f \pm \frac{\mathrm{P}k'}{\mathrm{M'P}} \times f' \pm \text{\&c.}\right) \times \dot{\mathrm{A}}$, from the two former may be deduced the equations $v\dot{v} = \alpha\dot{f} + \beta\dot{f}' + f\dot{\alpha} + f'\dot{\beta} + \dot{\gamma}$, and $v\dot{v} = \alpha'\dot{f} + \beta'\dot{f}' + f\dot{\alpha} + f'\dot{\beta}' + \dot{\gamma}'$, where $\alpha = \frac{\mathrm{C} \times \mathrm{PH}}{2\mathrm{MP}}$, $\beta = \pm \frac{\mathrm{C} \times \mathrm{PH}'}{2\mathrm{M'P}}$, $\gamma = \pm \frac{1}{2} \mathrm{C} \left(\frac{\mathrm{PH}'' \times f''}{\mathrm{M''P}} \pm \frac{\mathrm{PH}''' \times f'''}{\mathrm{M'''P}} \pm \text{\&c.}\right)$; $\alpha' = \frac{\mathrm{C}' \times \mathrm{P}b}{2\mathrm{MP}}$, $\beta' = \pm \frac{\mathrm{C}' \times \mathrm{P}b'}{2\mathrm{M'P}}$, $\gamma' = \pm \frac{1}{2} \mathrm{C}' \left(\frac{\mathrm{P}b'' \times f''}{\mathrm{M''P''}} \pm \frac{\mathrm{P}b''' \times f'''}{\mathrm{M'''P}} \pm \text{\&c.}\right)$, whence may be derived the two equations $\alpha\dot{f} + \beta\dot{f}' + f\dot{\alpha} + f'\dot{\beta} + \dot{\gamma} = \alpha'\dot{f} + \dot{f}' + f\dot{\alpha}' + f'\dot{\beta}' + \dot{\gamma}' = \pi f \pm \rho f' \pm \sigma$, where $\pi = -\left(\frac{\mathrm{PK}}{\mathrm{MP}} = \frac{\mathrm{P}k}{\mathrm{MP}}\right) \times \dot{\mathrm{A}}$, $\rho = \pm \left(\frac{\mathrm{PK}'}{\mathrm{M'P}} = \frac{\mathrm{P}k'}{\mathrm{M'P}}\right) \times \dot{\mathrm{A}}$, $\sigma = -\left(\pm \frac{\mathrm{PK}''}{\mathrm{M''P}} \times f'' \pm \frac{\mathrm{PK}'''}{\mathrm{M'''P}} \times f''' \pm \text{\&c.} = \text{\&c.}\right) \times \dot{\mathrm{A}}$.

Reduce theſe two equations to one, ſo that f', f'', &c. and their fluxions, may be exterminated, and there reſults a fluxional equation of the formula $\mathrm{H}\ddot{f} + \mathrm{K}\dot{f} + \mathrm{L}f + \mathrm{M} = o$, where H, K, L, and M, are functions of one of the before mentioned variable quantities (for example, MP = W) which may be ſuppoſed to flow uniformly, and its fluxion.

PROP. VII.

1. Fig. 6. Given the force tending to any point S, the velocity and direction of the body; to find the curve deſcribed.

Let the body acted on by a force f tending to S, at the diſtance D′ from S be projected in the direction PY′, with a velocity H; and let the perpendicular from S to the tangent P′Y′ be A; from the general fluent of $f \times \dot{D}$, where D denotes the diſtance from S, and f is a function of D, properly corrected find its velocity V at diſtance D, and conſequently the perpendicular SY from the center S to the tangent PY at diſtance D = SP, which will be $\frac{A \times H}{V} = SY$; but A and H are given quantities, and V a known function of D; therefore SY and $\sqrt{SP^2(D^2) - SY^2} = PY$ will be known functions of D; and from the ſimilar triangles SPY and PQT may be deduced PY : SY :: $PT = \dot{D}$: QT, and conſequently $SP \times QT = D \times \frac{\dot{D} \times SY}{PY}$ (which is a known function of D multiplied into $\dot{D}$) will be as the increment of the area deſcribed round the center of force, of which the fluent properly corrected is proportional to the area deſcribed round the center of force; and conſequently to the time. In like manner, $\frac{Sy \times \dot{D}}{D \times PY} = \frac{QT}{SP}$ (proportional to the increment of the angle deſcribed by the body round S) is a function of D multiplied into $\dot{D}$, of which the fluent properly corrected, or angle, will be as a function of D.

12. Fig. 7. Given the above-mentioned force, &c.; to find an equation expreſſing the relation between the abſciſs $SM = x$ and ordinate $MP = y$ of the curve deſcribed, and their fluxions.

From the ſimilar triangles Ppo and LPM can be deduced $po = \dot{y} : oP = \dot{x} :: PM = y : LM = \frac{y\dot{x}}{\dot{y}}$; but $LM \pm SM = \frac{y\dot{x}}{\dot{y}} \pm x = \frac{y\dot{x} \pm x\dot{y}}{\dot{y}} = LS$; and conſequently $Pp = \sqrt{\dot{x}^2 + \dot{y}^2} : po = \dot{y} :: LS = \frac{y\dot{x} \pm x\dot{y}}{\dot{y}} : SY = \frac{y\dot{x} \pm x\dot{y}}{\sqrt{\dot{x}^2 + \dot{y}^2}}$; but SY is a function to be deduced as above of $SP = \sqrt{x^2 + y^2}$, whence the fluxional equation $\frac{y\dot{x} \pm x\dot{y}}{\sqrt{\dot{x}^2 + \dot{y}^2}} = \phi : (x^2 + y^2)$.

2. Fig. 8. Let a body be acted on by any number of forces (f, f', f'', f''', &c.) in the ſame plane tending to the given points S, S′, S″, S‴, &c.; to find an equation expreſſing the relation between $SP = D$ and $S'P = D'$, and their fluxions, where P is a point ſituated in the curve which the body deſcribes.

Suppoſe YP a tangent to the curve at the point P, and PZ perpendicular to it; and reſolve all the forces tending to S, S′, S″, &c. reſpectively into two others; one in the direction PY, and the other in the direction PZ; ſubſtitute for SP, S′P, S″P, S‴P, &c. reſpectively D, D′, D″, D‴, &c.; and ſuppoſe SY, S′Y′, S″Y″, S‴Y‴, &c. perpendicular to the line PY; then will the triangles PQT and SPY, PQ′T′ and S′PY′ be ſimilar, where PQ denotes a very ſmall arc, and QT and QT′ are perpendicular to the lines SP and S′P; hence $PQ = \frac{PT \times SP}{PY} = \frac{\dot{D} \times D}{PY} = \frac{PT' \times S'P}{PY'} = \frac{\dot{D}' \times D'}{PY'}$; and conſequently $PY : PY' :: \dot{D} \times D : \dot{D}' \times D'$; and if the quantities D, D′, $\dot{D}$ and $\dot{D}'$ are given, the ratio of PY : PY′ will be given; which being given, together with the line $SS' = a$, the lines PY and PY′, SY and S′Y′, can be found; for, drawing SL parallel to PY, and meeting S′Y′ in L, let $PY' = m \times PY$, then $YY' = (m \pm 1)\,PY = SL$, $SY = \sqrt{(SP^2 - PY^2)} = \sqrt{(D^2 - PY^2)}$, $S'Y' = \sqrt{(S'P^2 - PY'^2)} = (D'^2 - m^2 \times PY^2)$, $LS' = S'Y' \pm SY = \pm \sqrt{(D'^2 - m^2 PY^2)} \pm \sqrt{(D^2 - PY^2)}$; and $SS'^2 = SL^2 + LS'^2$ an equation in which all quantities (except PY) are given, and conſequently PY is determined by an equation, which will be a quadratic; but PY being found, from thence PY′, SY and

$S'Y'$ may be deduced, which are conſequently all functions of D, D', $\dot{D}$, $\dot{D}'$, and invariable quantities; and their fluxions $P\dot{Y}'$, $S\dot{Y}$, and $S'\dot{Y}'$ functions of D, D', $\dot{D}$ $\dot{D}'$, $\ddot{D}$, and $\ddot{D}'$: from the ſimilar triangles before given $S\dot{Y} : PQ = \frac{D\dot{D}}{PY} :: PY : \frac{D\dot{D}}{S\dot{Y}} = PO$ the radius of curvature hence PO is a function of D, D', $\dot{D}$, $\dot{D}'$, and $\ddot{D}'$, if $\ddot{D} = o$; and from D, D', SS', $\dot{D}$, $\dot{D}'$, and the point S'' given in poſition can be determined $S''P$, $S''Y''$ and PY''; for let $S''b = C$ be drawn perpendicular to $SS' = a$, and $Sb = b$; then will Sl (if Pl be a perpendicular from the point P to the line SS') $= \mp \frac{a^2 + D^2 - D'^2}{2a}$, and $S'l = \pm \frac{a^2 + D'^2 - D^2}{2a}$, and $Pl = \sqrt{(SP^2 - Sl^2)}$, and $S''P = \sqrt{((b \pm Sl)^2 + (C \pm Pl)^2)}$; draw $S''Y''$ perpendicular to the tangent PY, and cutting the lines SS' and SK parallel to PY in o and n reſpectively; then will $ob = \frac{C \times \sqrt{(SS'^2 - (PY \pm PY')^2)}}{(PY \pm PY') = l}$, $S''o = \frac{C \times SS'}{l}$; (and from the ſimilar triangles $S''ob$ and Son) $on = (b \pm ob) \times \frac{ob}{S''o}$; whence $S''Y'' = \pm S''o \pm on \pm SY$ will be a known function of D, D', $\dot{D}$ and $\dot{D}'$, and invariable quantities: the ſame may be predicated of ſimilar lines drawn to the centers S''', S'''', &c.; and conſequently $(f \times \frac{PY}{SP} \pm f' \times \frac{PY'}{S'P} \pm f'' \times \frac{PY''}{S''P} \pm f''' \times \frac{PY'''}{S'''P} \pm \&c.) \times \dot{A}$ (where $\dot{A}$, as before, denotes the fluxion of the arc of the curve) $= f \times \dot{D} \pm f' \times \dot{D}' \pm f'' \times \dot{D}'' \pm f''' \times \dot{D}''' \pm \&c. = -v\dot{v}$, if v denotes the velocity; but as f, f', f'', f''', &c. are functions of D, D', D'', D''', &c. reſpectively, the fluent of the above mentioned quantity $f\dot{D} \pm f'\dot{D}' \pm f''\dot{D}'' \pm \&c.$ can be found in terms of D, D', D'',

D'', D''', &c. from the fluents of the fluxions $f\dot{D}$, $f'\dot{D}'$, &c.; and conſequently in terms of D and D′, which let be Z, then will $Z = \frac{-v^2}{2}$; but $v^2 = -2Z = PO \times (f \times \frac{SY}{SP} \pm f' \times \frac{S'Y'}{S'P} \pm f'' \times \frac{S''Y''}{S''P} \pm f''' \times \frac{S'''Y'''}{S'''P} \pm \text{\&c.})$ a fluxional equation of the ſecond order expreſſing the relation between D and D′, and their fluxions.

2. To find an equation expreſſing the relation between $x = SM$ and $y = MP$, where SM (x) is the abſciſs beginning from S and continued in the line SS′, and MP (y) the perpendicular ordinate of the curve deſcribed by a body acted on by the above mentioned forces: in the fluxional equation found before for D and D′ and their fluxions ſubſtitute $(x^2+y^2)^{\frac{1}{2}}$ and $((SS' \pm x)^2 + y^2)^{\frac{1}{2}}$ and their fluxions, and there reſults the equation ſought.

Cor. It eaſily appears, that the general fluent may contain two invariable quantities to be aſſumed at will, or according to the conditions of the problem; that is, at a given diſtance the velocity and the direction may be aſſumed at will, and conſequently the general fluxional equation expreſſing the above mentioned relation will be of the ſecond order, if no fluents are contained in it.

Cor. From Py and Py', and the points S and S′ being given, can eaſily be deduced geometrically the direction of the tangent and the lines Sy, Sy', &c.; for divide the line SS′ in r, ſo that $Py \pm Py' : SS' :: Py : Sr$, and through r draw the line Pr, the perpendicular to Pr through P will be the tangent yPy'; to this line the perpendiculars from S and S′ will be the lines Sy and $S'y'$ required.

Cor. From the fluent of the above-mentioned fluxional equation may be deduced the velocity V in terms of D and D′; and from the fluent of $\frac{D\times\dot{D}}{Py\times V}$, which is a function of D multiplied into $\dot{D}$, may be deduced the time.

3. If the plane in which the body (P) moves, and all the forces f', f'', f''', &c. tending to points M′, M″, M‴, &c. not ſituated in the ſame plane (except one f tending to a given point M) be given, then the force tending to that point can be found, and the curve deſcribed. Reſolve all the forces tending to the points M, M′, M″, M‴, &c. into two others; one MS, M′S, M″S, M‴S, &c. perpendicular to the plane in which the body moves, and the other SP, S′P, S″P, S‴P, &c. in the plane; then will $f\times\frac{MS}{MP}\pm f'\times\frac{M'S'}{M'P}\pm f''\times\frac{M''S''}{M''P}\pm\&c.=0$, from which equation f the force tending to the point M may be found; then, from the preceding propoſition find the curve, which a body agitated by forces $f\times\frac{SP}{MP}$, $f'\times\frac{S'P}{M'P}$, $f''\times\frac{S''P}{M''P}$, &c. tending to the points S, S′, S″, &c. deſcribes, and it will be the curve required.

4. If the body moves in a curve of double curvature, and the forces f, f', f'', &c. tending to all the centers M, M′, M″, M‴, &c. be given; from the fluent of the fluxional quantity $(f\times\frac{Py}{MP}\pm f'\times\frac{Py'}{M'P}\pm f''\times\frac{Py''}{M''P}\pm f'''\times\frac{Py'''}{M'''P}\ \&c.)\times\dot{A}$ (A denoting the ſame quantity as before) $=f\times M\dot{P}\pm f'\times M'\dot{P}\pm f''\times M''\dot{P}\pm f'''\times M'''\dot{P},\pm\&c.=f\times\dot{D}\pm f'\times\dot{D}'\pm f''\times\dot{D}''\pm f'''\times\dot{D}'''\pm\&c.=\dot{Z}=-v\dot{v}$ (f, f', f'', f''', &c. being given functions of D, D′, D″, &c. reſpectively) can be deduced the ſquare of the velocity $=-2z$, which will be a function of

D, D', D'', D''', D'''', &c., and conſequently a function of D, D', D'', eaſily to be derived: ſubſtitute this function $-2z$ for v^2 in the two following equations $\frac{v^2}{R'} = F'$ and $\frac{v^2}{R''} = F''$, where R' and R'' denote the radii of curvature in two different planes of which the tangent above mentioned in Prob. 4. art. 4. is their interſection, and F' and F'' the ſum of the forces in lines perpendicular to the tangent, and in the reſpective planes: from theſe forces, calculated in terms of the diſtances from three given points D, D', and D''; or in terms of two abſciſſæ and one ordinate, and from the radii R' and R'' may be deduced two fluxional equations of the ſecond order, expreſſing the relation between three diſtances D, D', and D'', &c. which may always be reduced to one fluxional equation of the fourth order expreſſing the relation between one abſciſs and its correſpondent ordinates, or the diſtances from two given points.

5. The general fluxional equation expreſſing the relation between the diſtances from two given points will be of the fourth order, if no fluents are contained in it; for it admits of four different quantities to be aſſumed at will, or according to the conditions of the problem.

6. If ſome points, to which the forces tend, are ſituated at an infinite diſtance; that is, ſome forces always act parallel to themſelves; from the given forces acting either to given points, or in parallel directions, by the equation $f \times \dot{D} \pm f' \times \dot{D}' \pm f'' \times \dot{D}'' \pm \&c. = -v\dot{v}$ can be deduced the ſquare of the velocity at a point P in terms of the diſtances from two given points, or of an abſciſs and ordinate; if the centers, &c. and parallel forces are all ſituated in the ſame plane: or in terms of the diſtances from three points, or two abſciſſæ and an ordi-

nate, if ſituated in different planes; from the centers, &c. and forces given, find the ſum F of the forces in any direction (PL) (the direction of the tangent excepted) acting on the body at the point P, and the chord of curvature C of the curve at the ſame point and in the ſame direction; in the equation $v^2 = \frac{1}{2} F \times C$ for v^2 ſubſtitute the value found before, and there reſults an equation expreſſing the relation between the diſtances from two points, or an abſciſs and ordinate, &c. if the forces act in the ſame plane: but if the forces act in different planes, find the ſum F and F′ of the forces at the point P in directions which are not both ſituated in one plane with the tangent and each other; and alſo the chords C and C′ of curvature in thoſe directions in terms of the diſtances from three points, or two abſciſſæ and one ordinate, &c. In the equations $v^2 = \frac{1}{2} F \times C$ and $v^2 = \frac{1}{2} F' \times C'$ for v^2 ſubſtitute its value found from the principles before given; and there reſult two fluxional equations of the ſecond order expreſſing the relation between the diſtances from three points, or two abſciſſæ and an ordinate, &c.

PROP. VIII.

Fig. 9. Let a body move in a curve P*p*, &c. and be acted on at P′ by a force *f* (which is as any function of the diſtance SP′) tending to S; let the velocities at P and *p* be repreſented by the lines YP and *yp* in the direction of the tangents to the points P and *p*; reſolve theſe forces YP and *yp* into two others Y*k* and *k*P, and *yl* and *lp*, of which one *k*Y and *yl* is parallel to the line SL; the other *k*P and *lp* is parallel to MP: let a body fall in the right line LS, and the force acting on the body at M′ be to the force acting on the body moving in the curve at P′ :: SM′ : SP′, and P′M′, PM

and *pm* be perpendicular to SL; then, if the velocity of the body falling in the right line SL at the point M be *k*Y, the velocity of the body at the point *m* acted on by the above mentioned forces will be *yl.*

This is eaſily demonſtrated from the reſolution of forces.

2. Through S draw SN parallel to PM or *pm*, &c., and aſſume in the line (SN) $SP = PM$ and $Sp = pm$, and let the force at P′ in the line SN and diſtance $= M'P'$: the force of the body moving in the curve at the diſtance $P'S : P'M' : SP'$; then if the velocity at the diſtance $SP = PM$ be P*k*, the velocity at the diſtance $Sp = pm$ will be *pl.*

Cor. The force in the direction of the line SL vaniſhes in the point where a perpendicular SN to the line SL paſſing through the point S cuts the curve, and conſequently the velocity in the direction of SL in that point is the greateſt or leaſt, &c.; but if the tangent of the curve be perpendicular in any point to LS, then the velocity in the direction LS is nothing: the ſame may be applied to the velocity in any other direction.

Ex. Fig. 10. Let a body move in the circumference of a circle SPA, of which the center of force is a point S in the circumference; it is known, that the force in the direction and at the diſtance SP is as SP^{-5}; but the force in the direction SP is by the hypotheſis to the force in the direction (SA) :: SP : SM, if PM be perpendicular to SM, and conſequently the force in the direction (SA) is as $SM \times SP^{-6}$; but if AS be a diameter, $AS \times SM = SP^2$; therefore $SM \times SP^{-6} = SM \times AS^{-3} \times SM^{-3} = \frac{SM^{-2}}{AS^3}$; and the diameter AS being given, the force in the line SA varies as SM^{-2}, that is, inverſely as the ſquare of the diſtance: if the force varies as $SM^{-2} = x^{-2}$, then $v\dot{v}$ will vary as

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$-\frac{\dot{x}}{x^2}$, where v denotes the velocity; and v^2 will vary as $\frac{1}{x}-\frac{1}{SA}$, which agrees with the ſquare of the velocity deduced from the preceding principles; for $v=PY$ the velocity at P is inverſely as the perpendicular $SY=SM$ let fall from the center of force on the tangent; but $SA^2 : 2SP \times PA ::$ velocity PY as $\frac{1}{SY}=\frac{1}{SM} : Pl$ the velocity at M; whence Pl^2 (the ſquare of the velocity at M) $=\frac{4SP^2 \times PA^2}{SA^4} \times PY^2$ which varies as $\frac{4SP^2 \times PA^2}{SA^4} \times \frac{1}{SM^2}=\frac{4PA^2}{SA^3 \times SM}=\frac{4SA^2-4SA \times x}{SA^3 \times x}$, and conſequently as $\frac{1}{x}-\frac{1}{SA}$ the ſame as above.

2. Fig. 9. If any number of forces act on a body at P in any given directions parallel, or tending to given points; reſolve all the forces into two others; one in a given direction SM, and the other in a direction PM perpendicular to it, of which let F be the ſum of the forces reſulting in the direction M*m*S, and *f* the ſum of the forces reſulting in the direction PM; reſolve the velocity V of the body at P, which is in the the direction of the tangent PY, into two others V' and V'', one in the direction parallel to the line SM, and the other perpendicular to it: in the ſame manner reſolve the velocity v of the body at *p*, which is in the direction of the tangent *py*, into two others v' and v'', one in the direction parallel to the line SM, and the other perpendicular to it: then if the velocity of the body moving in the right line SM at M be V', and it is conſtantly acted on by a force $=$ F, the velocity of the body at *m* will be v': and if the body move from P in a direction perpendicular to SM with a velocity as V'', and be always acted on by a force *f*, the velocity at the diſtance PM − *pm* will be v''.

Cor. From the forces given and the velocities in the above mentioned directions at the point P, can be deduced the velocities in the ſame directions at the point *p*, and conſequently the tangent to the curve at the point *p*.

PROP. IX.

1. Let the reſiſtance of a body, moving in a right line, be as any function V of the velocity v; then will $\dot{t} = \frac{\dot{v}}{V}$, $\dot{x} = \frac{-v\dot{v}}{V}$; where $\dot{t}$, $\dot{v}$, and $\dot{x}$, denote the increments of time, velocity, and ſpace; their fluents properly corrected will give the time and ſpace in terms of the velocity.

2. Let a body move in a right line, and be acted on by an accelerating force in that line, which varies as any function X of the diſtance x from a given point; and reſiſted by a force which is as any function V of the velocity v into its denſity X′, which varies alſo as a function of x and v; then will $(X + aVX')\dot{x} = -v\dot{v}$, from its fluent x can be found in terms of v, or v in terms of x; and thence $\dot{t} = \frac{\dot{v}}{X + aVX'}$, of which the fluent properly corrected gives the time.

Ex. 1. Let $V = v^2$ and X′ a function of x; that is, let the reſiſtance be as the ſquare of the velocity and denſity, whence $(X + av^2X')\dot{x} = -v\dot{v}$, of which equation the fluential will be

$$e^{\int 2aX'\dot{x}} \times \frac{v^2}{2} = -\int e^{\int 2aX'\dot{x}} \times X\dot{x} + A, \text{ and } t =$$

$$\int \frac{\dot{x}}{\sqrt{-(e^{-\int 2aX'\dot{x}} \times (\int e^{\int 2aX'\dot{x}} \times X\dot{x} + A))}} + B,$$ where A and B are invariable quantities to be aſſumed according to the conditions of the problem.

1.2.

1.2. Let $e^x = X'$ and $X = b$, which is ſuppoſed to correſpond nearly to the ſtate of our atmoſphere, then will $v^2 = -2 \times e^{-\int 2aX\dot{x}} \times \int e^{\int 2aX\dot{x}} \times X\dot{x} = -2 \times e^{-\int 2ae^x\dot{x}} \int e^{\int 2ae^x\dot{x}} \times b\dot{x} = -2e^{-2ae^x - b} \times \left(\int e^{2ae^x + b} \times b\dot{x} + A\right)$, e being the number, whoſe hyperbolic log. is 1, and b and A quantities to be aſſumed according to the conditions of the problem.

1.3. Let $X = X'$, and it becomes $X\dot{x} = \frac{-v\dot{v}}{1 + aV}$, and $\dot{t} = \frac{-\dot{v}}{X(1 + aV)}$.

2. Let X be an homogeneous function of one dimenſion of x, that is, $= ax$, and V a ſimilar function of n dimenſions of v, that is $= bv^n$, and X' a ſimilar function of r dimenſions of x and v, and $n + r = 1$; then by ſubſtituting zx and its fluxion for v and its fluxion, can be found the fluent of the fluxional equation $(X + aVX')\,\dot{x} = -v\dot{v}$, and conſequently the velocity and time by the quadrature of curves in terms of the ſpace; and in like manner of many other caſes.

3. Fig. 4. Let a body moving in a given curve be acted on at any point P by a force f tending to a given point S, and reſiſted by a medium proportional to V a function of its velocity multiplied into its denſity X' a function of the diſtance $SP = D$; to find its velocity, time, and diſtance from the given point S in terms of each other. Let $F = f \times \frac{PY}{SP}$ the force in the direction of the tangent PY, and conſequently $(F + VX')\,\dot{A} = -v\dot{v}$, and $v^2 = \frac{1}{2} C \times f$, where $\dot{A}$ is the increment of the arc, and C the chord of curvature in the direction SP; but ſince the curve is given, the chord of curvature may be deduced from the diſtance, &c. and the increment $\dot{A}$ of the arc from a function of the diſtance multiplied into the increment

of

of the diſtance; then, if f or v be a given function of the diſtance, the other may be deduced from it, and conſequently $-v\dot{v}=\varphi:(D)\times\dot{D}$ will be a given function of the diſtance D multiplied into $\dot{D}$, whence we have $\varphi:(D)\times\dot{D}=\dot{D}\,(f\times\frac{PY}{SP}+X'V)$ divide by $\dot{D}$, and there reſults an algebraical equation, from which $V\times X'$ may be found.

If neither v nor f be given, reduce the two equations $(f\times\frac{PY}{SP}+VX')\,\dot{A}=-v\dot{v}$ and $v^2=\frac{1}{2}Cf$ into one, ſo as to exterminate either f or v and its fluxions, and there reſults an equation expreſſing the relation between the other v or f and D and their fluxions: from the velocity given in terms of D may be deduced the time from the equation $\dot{t}=\frac{\dot{A}}{v}$.

3.2. If the body be acted on by forces tending to more points S, S′, S″, S‴, &c. in the ſame plane; reſolve each of the forces into two; one in the direction of the tangent, and the other perpendicular to it; let the ſum of the forces in the direction of the tangent be F; and in the direction perpendicular to it be F′; and 2R the diameter of curvature at the point P, which will be given in terms of the diſtances from two points, or of an abſciſs and ordinate, and their fluxions, &c.: aſſume the two equations before given $(F+X'V)\,\dot{A}=-v\dot{v}$ and $v^2=F'R$, and ſince $\dot{A}$ is always given in terms of D and $\dot{D}$, if F and F′ be given in terms of D, D′, &c. the value of $V\times X'$ may be acquired by a ſimple algebraical equation: but if F and F′ be not given, and conſequently v not given, but V a given function of v, and X′ a given function of the above mentioned diſtances; then ſubſtitute for v its value $\sqrt{(F'R)}$ in the function V, and the fluxion of $\frac{1}{2}F'R$ for $v\dot{v}$, and there will reſult an equation involving D and F′ and their fluxions, and F; but if the forces tending to all the points but one are given in terms of the diſtance D, or abſciſs or ordinate of the curve, and their fluxions; then from F′ can be found F, and, *vice verſá*, from F can be found F′, and conſequently there reſults a fluxional equation expreſſing the relation between F or F′ and the diſtance D or D′, &c. or abſciſs or ordinate, and their fluxions.

From F and F′, and conſequently v being found in terms of D, D′, &c. can be deduced $t = \frac{\dot{v}}{F}$.

The ſame method may be applied, if ſome forces tend to an infinite diſtance, that is, act parallel to themſelves, and others tend to given points.

Ex. Let the accelerating force be directly as the arc $= x$, and the reſiſtance uniform $= a$; then will $(x - a)\,\dot{x} = -v\dot{v}$, and conſequently $x^2 - 2ax + B = -v^2$; let A be the arc, where the velocity $= o$; then will the equation $A^2 - 2aA - x^2 + 2ax = v^2$, and the increment of the time $t = \frac{\dot{x}}{v} = \frac{\dot{x}}{\sqrt{(A^2 - 2aA - x^2 + 2ax)}}$, whoſe integral is $\frac{1}{A - a} \times$ arc of a circle, of which the radius is $A - a$ and coſ. $= x - a$, where A is the diſtance of the point from which the body begins to fall, and the loweſt point of the curve; and the accelerating force $x - a$ is as the diſtance from a point (a) of a curve, of which the diſtance from the loweſt is a.

Cor. The times of the body falling from any point of the curve to a will be equal.

Cor. The body on this hypotheſis will either reſt at the point a, or at the loweſt point, or any point between $+a$ and

$-a$; for it may reſt at any point, where the reſiſting force is always equal or greater than the accelerating force.

Cor. Let n be the number of vibrations, then the diſtance of the arc, to which it will aſcend from the loweſt point at n vibrations, will be $A - 2na$; if $A - 2na$ be not greater than $2a$, it will never paſs the loweſt point.

Philoſophical enquiries require ſome corrections, which do not enter into mathematical calculus; for example, in ſome caſes the calculus changes the quantities from negative to affirmative, &c. when from philoſophical conſiderations they are not changed; and, *vice verſâ*, they may be changed to affirmative, &c. on philoſophical conſiderations, when they are not changed from the calculus: and alſo a body may ſtop, &c. from philoſophical conſiderations, as in the preceding example, when it does not follow from the algebraical calculus, &c. It is further to be obſerved, that reſiſtances are always to be taken affirmatively.

Ex. 2. Let the accelerating force be as the arc, that is, the diſtance from the loweſt point, and the reſiſtance as the velocity; then will the fluxional equation $(F - V)\dot{A} = -v\dot{v}$ be $(ax - v)\dot{x} = -v\dot{v}$, which is an homogeneous equation of the firſt order: write in it zx for v, and its fluxion for $\dot{v}$, and there reſults the equation $(ax - zx) \times \dot{x} = -zx^2\dot{z} - z^2x\dot{x}$, whence $(a - z)\dot{x} = -zx\dot{z} - z^2\dot{x}$ and $\frac{\dot{x}}{x} = \frac{-z\dot{z}}{a - z + z^2}$, and thence log. $x =$ $-\frac{1}{2}$ log. $(a - z + z^2)$ (W) $- \frac{2}{4a-1} \times$ cir. arc, whoſe radius is $\frac{\sqrt{4a-1}}{2}$ and tangent $(z - \frac{1}{2}) + B$, whence can be found $v = xz$, and from curvilinear areas $t = \frac{\dot{x}}{v}$.

If $4a$ is leſs than 1, then it becomes $\log. x = W - \frac{1}{4\sqrt{\frac{1}{4}-a}} \times \log. \frac{x - \sqrt{\frac{1}{4}-a} - \frac{1}{2}}{x + \sqrt{\frac{1}{4}-a} - \frac{1}{2}} + B$; where B is an invariable quantity to be aſſumed according to the conditions of the problem.

Cor. If the force be directly as the diſtance, or as the arc of the curve from the body to the loweſt point, and the reſiſtance as the velocity; then will the velocity in one arc be to the velocity in the correſponding point of another arc, as the arcs to be deſcribed; and conſequently the times equal.

4. If the body is acted on by forces tending to points S, S′, S″, &c. ſituated in different planes, then let F be the ſum of the forces in the direction of the tangent at the point P; F′ and F″ the ſum of the forces acting on the body in two different directions at the ſame point, which are not ſituated in the ſame plane with the tangent and each other; from the three equations $(F + X'V)\dot{A} = -v\dot{v}$ and $\frac{v^2}{C} = \frac{1}{2}F'$ and $\frac{v^2}{C} = \frac{1}{2}F''$, in which the ſame letters denote the ſame quantities as before, and C and C′ denotes the chords of curvature in the ſame directions as the forces F′ and F″, which from the curve being given can be found at any point; and if F′ or F″ is given in terms of the diſtance* from a given point, or an abſciſs or ordinate, &c. the velocity v can be found in terms of the ſame, and X′V by a ſimple algebraical equation: if F′ is not given, and V is a given function of v, ſubſtitute in V for v its value $\sqrt{(\frac{1}{2} C \times F')}$, and there reſults an equation expreſſing the relation between F (which can be deduced from F′ or F″) and the diſtance of the body from ſome given point, or the abſciſſæ and ordinates of the curve required, and their fluxions.

If ſome of the forces act in parallel directions; the forces, velocities, &c. may be found by the ſame method.

PROP. X.

Fig. 11. Let a body be projected in a direction HL with a given velocity, and be acted on by a force in a direction parallel to AP $=x$, which varies as X a function of x; and alſo by another force in a direction parallel to MP $=y$, that is, perpendicular to AP, which force varies as Y a function of y; and let it move in a medium, of which the reſiſtance is proportional to the velocity; to find the curve deſcribed.

Find the fluent of $(X+av)\,\dot{x}=-v\dot{v}$, which corrected according to the conditions of the problem (*viz.* ſo that v at the point H may be to the velocity of projection :: Hc : Hb, where bc is drawn perpendicular to AP) ſuppoſe $v=X'$; find the fluent of $\frac{\dot{x}}{X'}$, which corrected ſo as to become $=o$, when $x=AH$, let be X''. In the ſame manner find the fluent of $(Y+a'v')\,\dot{y}=-v'\dot{v}'$, which corrected, ſo that v' at the point H may be to the velocity of projection :: cb : Hb, ſuppoſe $v'=Y'$; find the fluent of $\frac{\dot{y}}{Y'}$, which corrected ſo as to become $=o$, when $PM=o$, let be $=Y''$; aſſume $X''=Y''$, and thence from x find y: take $AP=x$ and $PM=y$, and M will be a point in the curve, which a body projected in the line HL deſcribes; and if Mm in the direction parallel to HAP : mo perpendicular to it :: velocity v : velocity v', then will Mo be a tangent to the curve in the point M.

2. If a body is acted on by forces tending to any given points S, S′, S″, &c. which vary as given functions of their diſtances from the body, and reſiſted by a force which varies according to a given function V of the velocity (v) into its denſity X′,

where

where X′ varies according to ſome function of the diſtances from the given points, &c.; to find the curve deſcribed.

1. From the diſtances of the body from two given points, or the abſciſs and ordinate of the curve deſcribed, and their fluxions, &c. find the forces acting in the direction of the tangent to the curve, and in ſome other direction, which ſuppoſe F and F′; and alſo the chord of curvature in the above mentioned direction, which let be C; then from the equations $(F+V\times X')\dot{A}=-v\dot{v}$ and $v^2=\frac{1}{2}C\times F$ reduced into one by writing for v its value in the function V, and for $v\dot{v}$ its value deduced from the equation $v^2=\frac{1}{2}C\times F$, and for $\dot{A}$ (the fluxion of the arc) its value deduced from the diſtances, &c. will reſult an equation expreſſing the relation between the diſtances from two given points to the curve, or its abſciſs and ordinates, and their fluxions.

3. If the forces are not all ſituated in the ſame plane, then from the before given equation $(F+V\times X')\dot{A}=-v\dot{v}$, and the two others $v^2=\frac{1}{2}C\times F'$ and $v^2=\frac{1}{2}C'F''$, where F denotes the force in the direction of the tangent, and F′ and F″ are the forces in different directions, which both are not ſituated in the ſame plane with each other and the tangent, and in which directions the chords of curvature are reſpectively C and C′; ſince the quantities F, F′, and F″; C and C′ and $\dot{A}$ (as proved before) can all be expreſſed in terms of the diſtances from three given points, or from two abſciſſæ and one ordinate, and their reſpective fluxions; may be deduced two fluxional equations expreſſing the relation between the diſtances from three given points, or two abſciſſæ and an ordinate, &c.

The ſame principles may be applied to caſes, in which ſome of the forces act in parallel directions.

On moveable Centers.

PROP. XI.

1. Given the reſpective places of (n) bodies S, S′, S″, S‴, &c. in the curves A, A′, A″, A‴, &c. at the ſame time, and in the ſame plane, and the forces of all the bodies acting on S, except two, S′ and S″; to find the forces of the two bodies S′ and S″ on the body S.

This propoſition may be reſolved by the method given in Prop. 4. for to produce the ſame effect the ſame finite forces will be requiſite, whether the centers of forces reſt or move in given curves.

1.2. If the bodies S, S′, S″, &c. move in different planes, then all the forces acting on the body, except three, may be given, which may be acquired from the method given in the ſame propoſition.

Hence it appears, that $2n$ forces may be requiſite to be found from the conditions of the problem to determine all the bodies to move in their reſpective curves, when they are all ſituated in the ſame plane, and that $3 \times n$ forces may be requiſite in different planes, &c. if the force of one body (S′) on another (S″) does not at all depend on the force of the ſame body (S′) on any other (S‴); and if the ſame can be prædicated of the reſt, then $n \,.\, \overline{n-3}$ forces of the above mentioned bodies in the ſame, or $n \,.\, \overline{n-4}$ forces in different planes may be aſſumed at will.

3. If the velocities v, v', v'', &c. at every point of the arcs a, a', a'', &c. of the (n) above mentioned curves A, A′, A″, &c. be given in terms of their arcs, abſciſſæ, or ordinates, &c. and the places in which the bodies are ſituated at the ſame time in the arcs b, b', b'', &c. of ſome other curves B, B′, B″, &c. find the correſponding velocities V, V′, V″, &c. at the ſame time of the bodies in the curves B, B′, B″, &c.; then make $\frac{\dot{a}}{v}=\frac{\dot{a}'}{v'}=\frac{\dot{a}''}{v''}=\&c.=\frac{\dot{b}}{V}$, or which is equal to it $=\frac{\dot{b}'}{V'}$ or $=\frac{\dot{b}''}{V''}=\&c.$ From the fluents of the fluxional equations reſulting properly corrected will be found the arcs a, a', a'', &c. deſcribed by the bodies in the curves A, A′, A″, &c. in the ſame time as the correſpondent arcs b, b', b'' &c.; and from thence, by the method given in the preceding caſe, may be deduced the forces.

The ſame principles may be applied to bodies moving in reſiſting mediums.

PROP. XII.

Given the law of the forces of two bodies acting on each other, to find the two curves by them deſcribed.

Fig. 12. Aſſume x and y for the abſciſs (AP) and ordinate (PM) of one curve, and z and u for the abſciſs (AP′) and ordinate (P′M′) of the other; where the abſciſſæ AP and AP′ begin from the ſame point A, and are ſituated in the ſame line; then will the diſtance (D = M′M) between the bodies $=\sqrt{\overline{z\pm x}^{2}}+\sqrt{\overline{u\pm y}^{2}}$); let the forces of the body placed at M on that at M′, and of the body placed at M′ on that at M vary as $\varphi:(D)=F$, and $\varphi':(D)=F'$; and let $Mp=\dot{x}$ and $pm=\dot{y}$; then will coſine of the angle mMM′ to radius (1) be $\frac{y\pm u}{D}\times$

$\dot{y}$

$\frac{\dot{y}}{\sqrt{(\dot{x}^2+\dot{y}^2)}} \pm \frac{x \pm z}{D} \times \frac{\dot{x}}{\sqrt{(\dot{x}^2+\dot{y}^2)}} = c$; and conſequently the force in the direction of the tangent M*m* will be $c \times F$, whence $-v\dot{v} = c \times F \times \sqrt{\dot{x}^2+\dot{y}^2}\ (\dot{A})$ and $v^2 = \frac{1}{2}CF$, where C is the chord of curvature in the direction of the force $(F) = \sqrt{1-c^2} \times 2\frac{(\dot{x}^2+\dot{y}^2)^{1\frac{1}{2}}}{\dot{y}\ddot{x}-\dot{x}\ddot{y}}$; and v the velocity of the body in the curve, whoſe abſciſs is x and ordinate y.

In the ſame manner let $\frac{x \pm z}{D} \times \frac{\dot{z}}{\sqrt{\dot{z}^2+\dot{u}^2}} \pm \frac{y \pm u}{D} \times \frac{\dot{u}}{\sqrt{\dot{z}^2+\dot{u}^2}} = c'$, the coſine of the angle made between the diſtance MM′ and arc of the curve of which the abſciſs is z and ordinate u, and conſequently $c' \times F'$ will be the force in the direction of its tangent, and therefore $-v'\dot{v}' = c' \times F' \times \sqrt{\dot{z}^2+\dot{u}^2}\ (\dot{A})$ and $v'^2 = \frac{1}{2}C'F'$, where C′ is the chord of curvature in the direction of the force $(F') = \sqrt{1-c'^2} \times 2\frac{(\dot{z}^2+\dot{u}^2)^{1\frac{1}{2}}}{(\dot{u}\ddot{z}-\dot{z}\ddot{u})}$, and v' the velocity of the body in the curve whoſe abſciſs is z and ordinate u; then, becauſe the times of deſcribing correſpondent arcs in the two curves are equal, their increments will be equal, and conſequently $\dot{t} = \frac{\sqrt{\dot{x}^2+\dot{y}^2}}{v} = \frac{\sqrt{\dot{z}^2+\dot{u}^2}}{v'}$; and there are deduced five fluxional equations, containing ſix variable quantities v, v'; x, y; z, and u, and their fluxions; reduce theſe equations, ſo that four of them (v, v', &c.) may be exterminated, and there will reſult an equation expreſſing the relation between x and y the abſciſs and ordinate of one curve, or z and u the abſciſs and ordinate of the other curve, and their fluxions; the fluential equation of which being found, and properly corrected, gives the equation to the curve.

The five equations are eaſily reduced to three by exterminating the quantities v and v'.

The fluxional equation reſulting will moſt commonly be of the fifth order, as evidently appears from the nature of the problem.

2. The ſame principles may be applied to determine the curves, when the bodies move in mediums, of which the reſiſtances are given: for example, ſuppoſe the reſiſtances to vary as a function of the diſtance from a given point into a function of the velocity: to the forces in the directions of the tangents contained in the preceding caſe muſt be added or ſubtracted the given reſiſtances for the forces in the directions of the tangents, and the remaining proceſs will be the ſame as is before given.

If two bodies deſcribe ſimilar orbits round a common center, either quieſcent or moving uniformly in a right line; the forces and velocities and reſiſtances of the medium will be to each other in correſpondent points as their reſpective diſtances from the center.

PROP. XIII.

Given the forces acting on any bodies, and tending to points either moveable or quieſcent, or in the direction of the tangents, &c.; to find the curve deſcribed by one of the bodies.

1. Aſſume x and y for the abſciſs and ordinate of the curve required, and from thence may be deduced the diſtances from any quieſcent center of force, and conſequently the force f in that direction; reſolve it into two others, one in the direction of the tangent, and the other in a different one; for example, let it be in a direction perpendicular to the tangent, and from their fluxions $\dot{x}$ and $\dot{y}$, and the force f may, by the method

before given, be deduced the forces in the two above mentioned directions; and in the ſame manner may be found from x, y, $\dot{x}$, and $\dot{y}$, the forces in the directions of the tangent and perpendicular to it, which follow from all the forces tending to given points, and acting on the body moving in the curve to be inveſtigated. 2. If ſome of the centers of force move in given curves B, B′, B″, &c. whoſe arcs let be denoted by B, B′, &c. and their reſpective places at the ſame time are given; then from their reſpective places given and forces, and x and y, and $\dot{x}$ and $\dot{y}$, can, as before, be deduced the forces in the direction of the tangent and its perpendicular to the curve required. 3. If other centers of forces move in given curves A, A′, A″, &c. and the velocities are given at every point of the curves; let A, A′, A″, &c. be the arcs of the curves A, A′, A″, &c. and ſuppoſe v, v', v'', &c. their correſpondent velocities; then, if the increments of the time be given, will $\frac{\dot{A}}{v}=\frac{\dot{A}'}{v'}=\frac{\dot{A}''}{v''}=$&c. but as the velocities are given at every point of the curves, v in the curve (A) will be given in terms of its abſciſs, ordinate, arc, &c. and conſequently $\frac{\dot{A}}{v}$ in terms of the ſame quantities and their firſt fluxions; the ſame may be affirmed of the fluxions $\frac{\dot{A}'}{v'}$, $\frac{\dot{A}''}{v''}$, in the curves A′, A″, &c.; hence, from the equation $\frac{\dot{A}}{v}=\frac{\dot{A}'}{v'}$, can be deduced the relation between the abſciſs or ordinate, &c. of the curve A and its correſpondent abſciſs or ordinate, &c. of the curve A′; and ſo of the remaining curves; hence this caſe is reduced to the preceding; but it is neceſſary alſo, that the times of the bodies in the two caſes ſhould be the ſame, in order that the places may correſpond, and conſequently $\frac{\dot{A}}{v}=\frac{\dot{B}}{V}$, where V denotes the velocity of the body at any point of the curve B from which equation can be deduced the correſpondent abſciſſæ and ordinates, &c. of the curves B and A; and thence the two caſes are reduced to the preceding, whence the correſpondent forces in the directions of the tangent, and perpendicular to it, can be found as above. 4. If ſome (m) of the centers move in curves L, L′, L″, &c to be deduced from the laws of the forces being given which act on them; aſſume z and u, z' and u', z'' and u'', &c. for their reſpective abſciſſæ and correſpondent ordinates; and from them and y and x, $\dot y$ and $\dot x$, find the forces acting on the body moving in the curve required in the direction of the tangent, and perpendicular to it, as before; then add all the forces deduced which act perpendicular to the tangent and alſo all contained in the direction of the tangent together with the reſiſting force in the ſame direction, and let the ſums reſulting be reſpectively F and F′: by the ſame method find the ſum of the forces which act on the bodies moving in L, L′, L″, &c. in the directions of the tangents, and perpendiculars to them, which ſuppoſe S and s, S′ and s', S″ and s'', &c.; then reduce the $2\,(m+1)$ equations of the formulæ found above, *viz.* $v^2 = F \times \frac{\overline{\dot x^2+\dot y^2}^{\,\frac{3}{2}}}{\dot y\ddot x - \dot x\ddot y}$ and $-v\dot v = F'\sqrt{\dot x^2+\dot y^2}$; $v'^2 = s \times \frac{\overline{\dot z^2+\dot u^2}^{\,\frac{3}{2}}}{\dot u\ddot z - \dot z\ddot u}$ and $-v'\dot v' = S \times \sqrt{\dot z^2+\dot u^2}$; $v''^2 = s' \times \frac{\overline{\dot z'^2+\dot u'^2}^{\,\frac{3}{2}}}{\dot u'\ddot z' - \dot z'\ddot u'}$ and $-v'' \times \dot v'' = S' \times \sqrt{\dot z''^2+\dot u''^2}$, &c.; where v, v', v'', v''', &c. reſpectively denote the correſpondent velocities of the bodies moving in the curves, whoſe abſciſſæ are x, z, z', z'', &c.; and alſo the $(m+1)$ equations $\frac{\dot B}{V} = \frac{\sqrt{\dot x^2+\dot y^2}}{v} = \frac{\sqrt{\dot z^2+\dot u^2}}{v'} = \frac{\sqrt{(\dot z'^2+\dot u'^2)}}{v''} = \frac{\sqrt{(\dot z''^2+\dot u''^2)}}{v'''}$ = &c. containing the $3\,(m+1)$

$+1$ variable quantities x and y, z and u, z' and u', z'' and u'', &c., v, v', v'', &c., and the variable quantity contained in B and V, into one, ſo that all the variable quantities except x and y and their fluxions may be exterminated, and there reſults an equation to the curve required expreſſing the relation between x and y its abſciſs and ordinate, and their fluxions.

5. If the forces are not ſituated in the ſame plane, aſſume X, x and y, for the two abſciſſæ and ordinates of the curve required; and Z, z and u; Z′, z' and u'; Z″, z'' and u''; &c. for the two abſciſſæ and ordinates of the (m) curves L, L′, L″, &c. reſpectively; and from the preceding method may be acquired the $3(m+1)$ equations $v^2=F\times C$, $v^2=F'\times C'$, and $-v\dot{v}=F''\times\sqrt{\dot{X}^2+\dot{x}^2+\dot{y}^2}$; $v'^2=S\times C'$, $v'^2=\sigma c'$ and $-v'\dot{v}'=s\times\sqrt{\dot{Z}^2+\dot{z}^2+\dot{u}^2}$; $v''^2=S'C''=\sigma'c''$ and $-v''\dot{v}''=s'\times\sqrt{(\dot{Z}'^2+\dot{z}'^2+\dot{u}'^2)}$; $v'''^2=S''C'''=\sigma''c'''$ and $-v'''\dot{v}'''=s''\times\sqrt{(\dot{Z}''^2+\dot{z}''^2+\dot{u}''^2)}$; &c.; in which v denotes the velocity in the required curve, and v', v'', v''', &c. the correſpondent velocities in the curves L, L′, L″, &c.; and F, F′, and F″; S, σ and s; S′, σ' and s'; S″, σ'' and s''; &c. denote the forces acting on the reſpective bodies in two different planes and in the tangents, which planes cut each other in the tangents of the curves; and C and c, &c., C′ and c', &c., C″ and c'', &c. the $\frac{1}{2}$ chords or radii of curvature in thoſe two planes to the different curves in the directions of the forces; and alſo the $(m+1)$ equations before mentioned $\frac{\dot{B}}{V}=\frac{\sqrt{\dot{X}^2+\dot{x}^2+\dot{y}^2}}{v}=\frac{\sqrt{\dot{Z}^2+\dot{z}^2+\dot{u}^2}}{v'}=\frac{\sqrt{Z'^2+\dot{z}'^2+\dot{u}'^2}}{v''}=$&c.; where $\sqrt{\dot{X}^2+\dot{x}^2+\dot{y}^2}$, $\sqrt{Z'^2+\dot{z}'^2+\dot{u}'^2}$, &c. are the fluxions of the arcs of the required curve, and of the curves L, L′, L″, &c. reduce theſe $4m+4$ equations con-

taining $4m+5$ variable quantities into two, ſo that all the variable quantities except three, X, x, and y, and their fluxions may be exterminated; and there reſult the two equations required.

It may be obſerved, that when the reſiſtance ariſing from the denſity of the medium and velocity (v) of the body varies as $X' \times v^2 + X$, where X and X′ are as functions of the diſtances from given points, the reſolution of the fluxional equations will generally be more eaſy, than when the reſiſtance varies as other functions of the velocities.

If the force acts equally on the particles of the body and fluid, then the force by which a body deſcends in a medium is as the whole force X acting on the body at the given diſtance multiplied into a fraction whoſe numerator is the difference between the denſity of the body (D) and fluid (X′) at that diſtance and denominator D, that is, as $X \times \frac{D - X'}{D}$.

Many caſes might have been given, in which the fluxional equations could have been reſolved; but in general their fluents can only be found by means of converging ſerieſes.

Fig. 1.

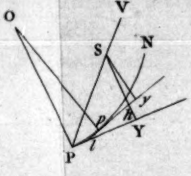
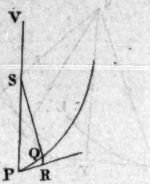


Fig. 2.



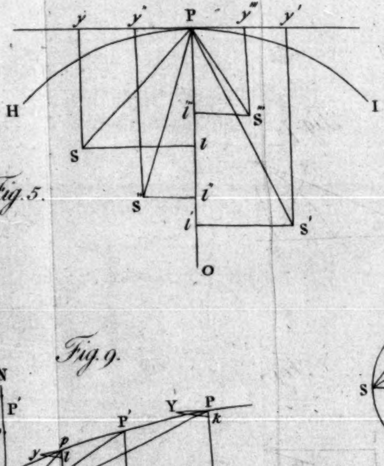


Fig. 5.

Fig. 9.

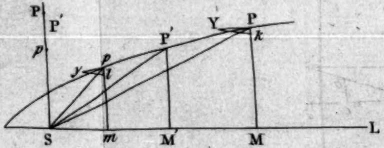


Fig. 3.

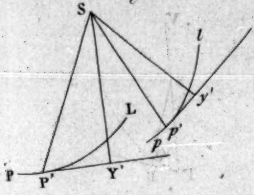


Fig. 4.

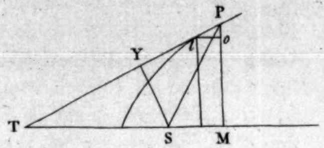


Fig. 6.

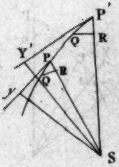


Fig. 7.

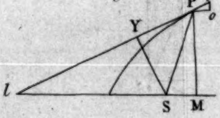
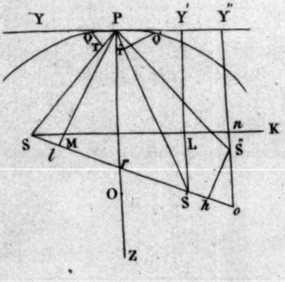


Fig. 8.



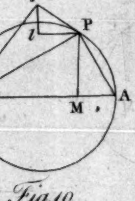
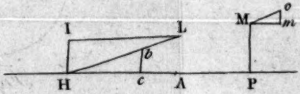


Fig. 10.

Fig. 11.



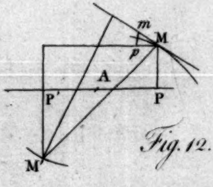


Fig. 12.

VIII. *Experiments on local Heat. In a Letter from* James Six, *Eſq. to the Rev.* Francis Wollaſton, *LL.B. F. R. S.*

Read January 10, 1788.

DEAR SIR,

THE following experiments are a continuation of thoſe I had the pleaſure of communicating to you ſome time ſince, relating to the diverſity of local heat in the atmoſphere; and confirm, in a more particular manner, my former obſervations reſpecting a remarkable refrigeration, which, in clear weather, takes place near the earth; for, although its ſurface in the day-time is then moſt liable to be heated by the ſun, yet after that is ſet, and during the night, the air is always found coldeſt near the ground, particularly in vallies. This phænomenon, for want of nocturnal obſervations, has been but lately noticed*; probably becauſe in the day-time, eſpecially when

the

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* Within theſe few years.

Mr. Wilson, in the Philoſophical Tranſactions, 1780, p 467. and 1781, p. 368. deſcribing the effects of a ſevere froſt at Glaſgow, mentions a remarkable refrigeration, which he perceived to take place on the ſurface of the ſnow and hoar-froſt, which ſunk the mercury in a thermometer laid upon it many degrees lower than one ſuſpended twenty-four feet above it. This phænomenon, he ſays, *had recently come under obſervation*; but ſuppoſing (I preſume) the principle of refrigeration to operate only on the ſurface of the ſnow, or hoar-froſt, when the cold was ſevere, it does not appear he made experiments at any other time.

In

the weather is ſtill and warm, the difference of temperature is reverſed, the reflection of the ſun's rays from the earth heating the air more below than it does above.

The experiments in my former Paper * were made partly in autumn, and partly in winter; and, the local variations differing in ſome meaſure with the ſeaſons, I was deſirous of continuing a ſeries of experiments throughout one entire year. To this end, therefore, I ſuſpended proper thermometers † in a ſhady northern aſpect, in the open air, at different heights; one in my garden at nine feet, and another in the Cathedral Tower 220 feet from the ground; continuing my journal, with the omiſſion of a few days only, from July 1784 till July 1785.

In the ſummer of 1783, making experiments with thermometers, which I had conſtructed to ſhew the greateſt degrees of heat and cold that happened in the obſerver's abſence, *I firſt perceived* this nocturnal refrigeration in the loweſt ſtratum of the atmoſphere; for by ſuſpending theſe thermometers at different altitudes, and viewing them only once in twenty-four hours, I found the true maximum and minimum of the heat and cold, which had happened during that time in their ſeveral ſtations.

A writer in the Gentleman's Magazine, for March 1785, p. 170, who did me the honour to mention my former Paper, containing *Experiments on Local Heat*, obſerved, that the effects of a ſharp froſt, which happened in *December of the preceding year*, appeared to be much more ſevere among the vegetables in the vallies than on the hills; and by taking the difference of temperature with thermometers at that time, he found them vary from 5 to 17 degrees, according to their ſeveral ſtations.

M. de Saussure, in his Voyages dans les Alpes, Vol. II. publiſhed 1786, ſays, " Quant à l'obſervation bien curieuſe *et bien nouvelle* de M. Pictet, que pendant la nuit la couche d'air la plus baſſe depuis la terre juſques a 5 pieds au-deſſus eſt plus froide que les couches ſuivantes depuis 5 pieds juſque a 50."

* Philoſophical Tranſactions, Vol. LXXIV. p. 428.

† Conſtructed to give the true maximum and minimum that may happen in the obſerver's abſence. See Philoſophical Tranſactions, Vol. LXXII. p. 72.

The reſult entirely correſponded with what I had before obſerved reſpecting the nocturnal diminution of heat, and the particular ſtate of the atmoſphere requiſite to produce it. The greateſt variations which happened within the year were in October and June; in the former month, the thermometers generally differed moſt in the night; in the latter, moſt in the day. From the 25th to the 28th of October, the heat below, in the night, exceeded, in a ſmall degree, the heat above, at which time there was frequent rain, ſometimes mingled with hail. From the 11th to the 14th, and alſo on the 31ſt, there was no variation at all; during which time likewiſe the weather was rainy; all the reſt of the month proving clear, the air was found colder below than it was above, ſometimes 9 or 10 degrees. In the month of June, the greateſt nocturnal variations happened from the 11th to the 15th, and from the 25th to the 30th; at both which times there appeared to be two currents of wind, the upper current from the S.W., the lower from the N.E.*. On cloudy nights, the loweſt thermometer ſometimes ſhewed the heat to be a degree or two warmer than the upper one; but in the day time the heat below conſtantly exceeded the heat above more than in the month of October. See Tab. I. and II. †.

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* Sometimes theſe were rendered viſible by clouds in different ſtrata, moving in different directions, and at other times by clouds moving in a contrary direction to a very ſenſible current of air below.

† The obſervations of the wind and weather were taken at Canterbury. In all the tables, the nocturnal degrees of cold belong to the night immediately preceding the day to the date of which they are placed.—With my thermometer I have ſometimes ſuſpended hygrometers; but their movement, although as conformable as poſſible to the moiſture of the air, had not a ſufficiently regular correſpondence with the variation of the thermometers to be inſerted in the tables

Being deſirous of knowing whether the nocturnal refrigeration increaſed on a nearer approach to the ſurface of the earth, I placed, in the midſt of an open meadow, on the bank of the river, near this city, two thermometers; one on the ground, and the other ſix feet above it; with theſe, and the two others before mentioned, one on the tower, and the other in my garden, I made obſervations from the 10th to the 23d of October, 1786. See Tab. III. But as the thermometer ſix feet from the ground, in the meadow, nearly agreed with that in the garden at nine feet, I have omitted an account of the latter in this table of compariſon. Here I found, as before, the nocturnal variations entirely regulated by the clearneſs, or the cloudineſs, of the ſky; and although they did not always happen in the ſame proportion to the reſpective altitudes, yet, when the thermometers differed at all, that on the ground was always the coldeſt.

Finding ſo conſiderable a difference as three degrees and a half, within ſix feet of the earth's ſurface, I increaſed the number of my thermometers in the meadow to four; one of them I ſunk in the ground, another I placed juſt upon the ground, a third I ſuſpended at three feet, and a fourth at ſix feet from the ground; at the ſame time I placed three thermometers in an open garden on St. Thomas's Hill, where the land is level with the Cathedral Tower, and about a mile diſtant from it; here I likewiſe put one in the ground, another juſt upon it, and ſuſpended a third ſix feet above it.

tables of compariſon. It is true, when the weather was clear and not too cold, the upper ones indicated dryneſs more than the lower; but in froſty weather, the contraction and expanſion of the hygrometers being impeded by the froſt, they did not in any regular manner agree with the variations of the thermometer.

With theſe ſeven thermometers and the two, before mentioned, in the city, I continued a diary for twenty days, taking alſo every morning the temperature of the water in the river; but the weather proving cloudy ſoon after, the thermometers hardly varied at all, ſeven or eight days only excepted. After this time I never rectified them but when the appearance of the weather gave me reaſon to expect that they would vary conſiderably. See the obſervations that were then taken Tab. IV.; by which it appears, that the cold in the night was generally greater in the valley than that on the hill; but that the variations between the thermometers on the ground, and thoſe ſix feet above them, were often as great on the hill as in the valley.

Perceiving now, that a difference of temperature was frequently found within three feet from the ground, I reſolved to try ſtill nearer; but my thermometers being eighteen inches in height could not be applied ſo near to the ſurface of the earth as my experiment required; I therefore conſtructed two others for the particular purpoſe; and by bending down the large tube, the bulb or body of the thermometer, to an horizontal poſition, while the ſmaller tubes remained in a vertical one, I was enabled to aſcertain the different degrees of heat from the ground to a ſingle inch.

At certain times, when the weather was clear, I expoſed theſe two horizontal thermometers in an open place, ſuſpending one of them, ſo as to bring the body of it within an inch of the ground, and the other nine inches above it. When the variation among the other thermometers was conſiderable, I found alſo a difference between theſe; the lower one indicating ſometimes more than two degrees leſs heat than the upper one, although they were placed ſo near to each other. See Tab. V.

From the foregoing experiments it appears, that a greater diminution of heat frequently takes place near the earth in the night-time, than at any elevation in the atmoſphere within the limits of my inquiry; and that the greateſt degrees of cold are at ſuch times always found neareſt to the ſurface of the earth; that this is a conſtant and regular operation of nature, under certain circumſtances and diſpoſitions of the atmoſphere, and takes place at all ſeaſons of the year; that this difference never happens in any conſiderable degree but when the air is ſtill, and the ſky perfectly unclouded*; but the moiſteſt vapour, ſuch as dews and fogs †, did not, as far as I could perceive, at all impede, but rather increaſe the refrigeration. In very ſevere froſts, when the air frequently depoſits a great quantity of frozen vapour, I have generally found it greateſt ‡; but the exceſs of heat, which in day-time, in the ſummer ſeaſon, was found at the lower ſtation, in the winter diminiſhed almoſt to nothing.

The foregoing experiments related to the difference of heat, which, at certain times, is found at different altitudes; the following to the different degrees of heat obſerved at different ſituations in reſpect to the ſea-ſhore. Tab. VI. and VII. contain a ſet of correſponding obſervations; among which are thoſe you were ſo obliging as to take for me at Chiſlehurſt[*]; others at the ſame time were taken in my garden, and on the Cathedral Tower; and others on the ſea-ſhore, about ſeven miles N.N.W. from Canterbury, where the thermometer was ſuſpended about 40 feet above high-water mark, 14 from the ground, and about 100 yards from the ſea[†].

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* Free from clouds which float higher in the air than fogs, and are ſufficiently opaque to hide the ſtars from our view even in the zenith.

† When fogs covered the valley, the refrigeration below was conſiderable; but they very ſeldom reached ſo high as the upper thermometer, and the ſky was clear above. No variation during rain, except mingled with hail.

‡ This remarkable diminution of heat near the ſurface of the earth in clear weather, after the ſun is ſet, and in the night, proceeds, I apprehend, partly from the coolneſs which the dews or vapours poſſibly may acquire in their deſcent, and partly from the evaporation which takes place at the ſame time from all moiſt or frozen bodies expoſed to the open air, particularly from thoſe on which the dew or hoar froſt is depoſited.

By Tab. VI. it appears, that every night, one only excepted, during that time, the air was coldeſt at Chiſlehurſt; and that the mean heat at the ſea-ſhore was equal to that on the tower at Canterbury. In the month of June (Tab. VII.), it is remarkable, that the cold was ſtill greater in the night at Chiſlehurſt than at any of the other places, excepting where there appeared two currents of wind, the upper current from the S.W. and the lower from the N.E.; at which time alſo there was the greateſt difference between the thermometer in the garden and that on the tower.

Tab. VIII. contains obſervations taken only at particular times, when the weather was extremely cold; and ſhews how nearly at ſuch times the temperature in the night, at the ſea-ſhore, generally agreed with that on the Cathedral Tower; with this exception, however, that on the 18th, 21ſt, and 22d of February, 1784, a little ſnow falling at ſeveral times in the day, the evenings after being clear, and the wind at N.E., the thermometer on the ſea-ſhore, contrary to what uſually happened, gave the cold greater than the thermometer on the tower; but under ſimilar circumſtances I have ſince ſeen the ſame.

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[*] The thermometer at Chiſlehurſt was ſuſpended twelve feet from the ground.

[†] The thermometer on the Cathedral Tower was elevated about 200 above that on the ſea-ſhore.

The following experiments relate to the variation of local heat in the earth itſelf; the diverſity of which appears from the different heat of the water iſſuing from it at different places *. It has been conjectured, that the diverſity of the temperature of ſprings may probably depend on their different elevations in the earth, with reſpect to the level of the ſea.

Two remarkably deep wells, both near the ſea-ſhore, and not far diſtant from Canterbury, gave me a favourable opportunity of making experimental inquiry into this matter; eſpecially as the ſituation of the two ſprings differed conſiderably from each other in reſpect to the level of the ſea. One of theſe is a well in Dover Caſtle, which is ſunk 360 feet through the high cliff of chalk on which the Caſtle ſtands, and the depth of the well is nearly equal to the height of the cliff from the ſea. The other is King's-Well at Sheerneſs †, which was ſunk 330 feet through almoſt one entire ſtratum of firm clay, where the ſurface of the ground is only four feet above high water.

Suppoſing therefore the ſpring in Dover well to lie level with the ſea, the ſpring of the well at Sheerneſs lies 326 feet below it; a circumſtance extremely favourable to my experiment.

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* The temperature here meant is not that of the very hot ſprings which proceed from particular local cauſes, ſometimes riſing near the ſurface of the earth; nor of ſprings which are near enough to the ſurface of the earth to be affected by the change of temperature in the atmoſphere; but that heat which being found at a great depth is more likely to be permanent, and may be ſuppoſed to originate from ſome general cauſe in the earth.

† See an account of ſinking this curious well by Sir Thomas Page, Phil. Tranſ. Vol. LXXIV. p. 6.

ment. The temperature of the ſprings I took in the following manner.

After fathoming each well with a line and plummet, I let one thermometer down to the bottom, and fixed another on the line, ſo as to reach to half the depth only, keeping a third to take the temperature of the air at the top.

Degrees of heat in the wells.

Sept. 28, 1784. Temperature of the water in the new well in Dover Caſtle.	Oct. 6, 1784. Temperature of the water in King's Well at Sheerneſs.
By thermometer at the top 56°	By thermometer at the top 53°
By ditto at the middle 52	By ditto at the middle 51
By ditto at the bottom 48½	By ditto at the bottom 56
Found the well 360 feet deep with 21 feet water.	Found the well 280 feet deep * with 180 feet water.

About noon was the time of day when I made the experiments at both places, and the top of the reſpective wells varying from each other depended wholly on the accidental temperature of the atmoſphere at the time; but that the thermometer at half the depth of the well at Dover gave nearly the mean heat of the top and bottom, while that in a correſponding ſituation in the well at Sheerneſs gave it colder than either top or bottom, I attribute to the following circumſtance.

Over the well at Sheerneſs a machine is erected, which raiſes the water by means of an horizontal windmill, working an endleſs chain. This chain, conſiſting of jointed double bars, with a number of buckets fixed thereon, at certain diſtances from each other, continually deſcending into, and aſcending out of the water, to an elevation of eight or nine feet above the

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* The ſand brought up from the bottom of the well by the force of the ſpring has reduced it to its preſent depth.

the top of the well, may be ſuppoſed to reduce the water as far as it reaches to the mean temperature of the air above; and thus I found it; for 51 degrees had been the mean temperature of the air near the ſea-ſhore for ſeveral days before. At the bottom of the well, near to which the chain never deſcends, I found the temperature 56 degrees; above 7 degrees warmer than that at Dover well.

The water at the bottom of theſe wells is, I preſume, too deep beneath the ſurface of the earth ever to be affected by the temperature of the atmoſphere; for if the heat of the ſummer could have had any influence on either of them, that at Dover muſt have been moſt conſiderably affected by it, eſpecially in the month of September; and the air was ſomething warmer when the experiment was made at Dover than at Sheerneſs. From the nature of the different kinds of ſtrata in which theſe wells are dug, had they been in all other circumſtances the ſame, one might reaſonably expect to find the warmer ſpring in the chalk, and the colder in the clay; but here the reverſe is ſeen, without any apparent local cauſe, except the different elevations of the ſprings in reſpect to the level of the ſea.

I am, &c.

JAMES SIX.

Canterbury,
Auguſt 1, 1787.

TABLE

TABLE I.

Thermometers at different ſtations compared together.

1784	Night.			Day.			Morning.	After-noon.	Evening.	Wind.	Barometer.
	In the garden.	On the tower.	Difference.	In the garden.	On the tower.	Difference.					
Oct.	°	°	°	°	°	°					In.
1	40½	45	− 4½	56	53	+3	Partly clear.	– –	– –	NE	30.2
2	42	46	− 4	58	56	+2	Cloudy.	– –	Clear.	E	30.3
3	40	45½	− 5½	55½	54	+1½	Partly clear.	– –	Cloudy.	E	30 4
4	40	46	− 6	55½	54	+1½	Partly clear.	– –	– –	E	30.2
5	41	51	−10	57½	56	+1½	Partly clear.	– –	Clear.	E	29.9
6	41	47	− 6	56	55	+1	Clear.	– –	– –	E	30.1
7	40	46	− 6	54½	52½	+2	Clear.	– –	– –	E	30.1
8	40	46½	− 6½	55	53	+2	Clear.	– –	– –	E	29.9
9	39½	43	− 3½	53	51	+2	Cloudy.	– –	Partly clear.	E	29.9
10	44½	45	− 0½	50½	49	+1½	Cloudy.	– –	Rain.	E	29.9
11	44	44	———	54	51	+3	Showery.	– –	– –	NW	30.0
12	44	44	———	55½	53	+2½	Showery.	– –	Cloudy.	NE	30.2
13	45	46	− 1	56	55	+1	Showery.	– –	– –	NE	30.2
14	44	44	———	50	48½	+1½	Cloudy.	Clear.	– –	E	30.1
15	31½	41	− 9½	50½	49	+1½	Clear.	– –	– –	NE	30.0
16	28½	38½	−10	53	51½	+1½	Clear.	– –	– –	E	30.0
17	28	38½	−10½	54	52½	+1½	Partly clear.	– –	– –	E	30.0
18	29	38½	− 9½	56	53	+3	Clear.	– –	– –	E	29.9
19	34	40	− 6	58	54	+4	Partly clear.	– –	Clear.	NE	29.7
20	30½	37	− 6½	54½	51½	+3	Clear.	Cloudy.	Clear.	NE	29.9
21	33	35	− 2	51	52	−1	Clear.	– –	– –	NE	30.1
22	43½	44	− 0½	55½	54	+1½	Cloudy.	– –	Partly clear.	SW	30.0
23	43	46	− 3	56	54	+2	Miſty rain.	– –	Cloudy.	W	29.7
24	32½	34	− 1½	45½	43	+2½	Clear.	– –	Rain.	NW	29.4
25	31	30½	+ 0½	43	40½	+2½	Cloudy.	– –	– –	NW	29.7
26	35½	32½	+ 3	44	43½	+0½	Cloudy.	Rain with hail.	– –	NE	29.9
27	40	39	+ 1	47	46	+1	Rain.	– –	– –	NE	30.0
28	42	41	+ 1	47½	46	+1½	Rain.	– –	– –	NE	30.0
29	40	42	− 2	47	48	−1	Cloudy.	– –	Rain.	NE	30.0
30	40	40	———	51	50	+1	Cloudy.	– –	Rain.	N	29.9
31	45	44	+ 1	50	49	+1	Cloudy.	– –	– –	W	29.8

TABLE II.

Thermometers at different ſtations compared together.

1785	Night.			Day.			Morning.	Afternoon.	Evening.	Wind.	Barometer.
	In the garden.	On the tower.	Difference.	In the garden.	On the tower.	Difference.					
June											In.
1	47°	46°	+1°	65°	59°	+ 6°	Showery.	— —	— —	W	29.8
2	47	48	−1	65	61	+ 4	Clear.	Cloudy.	— —	N	29.8
3	46	47	−1	71	67	+ 4	Partly clear.	— —	Clear.	SW	29.9
4	42½	46	−3½	70	66	+ 4	Partly clear.	— —	Clear.	SW. NE	29.8
5	54	54	—	68	60	+ 8	Partly clear.	Cloudy.	Rain.	W	29.7
6	51	49	+2	61	57	+ 4	Cloudy.	— —	— —	S	30.0
7	43	45	−2	62	58	+ 4	Cloudy.	— —	Rain.	SE	30.0
8	53½	52½	+1	73	66	+ 7	Rain.	— —	Showery.	SSW	30.0
9	56	55	+1	75	70	+ 5	Partly clear.	— —	— —	SW	30.2
10	56	55	+1	78	74	+ 4	Clear.	— —	— —	W	30.3
11	54	57½	−3½	84	74	+10	Clear.	— —	— —	SW	30.4
12	53	57	−4	71	63	+ 8	Clear.	— —	— —	SW. NE	30.3
13	41	49	−8	67½	64	+ 3½	Clear.	— —	— —	SW. NE	30.2
14	42½	50½	−8	69	64	+ 5	Clear.	— —	— —	SW. NE	30.2
15	48	53	−5	78	73	+ 5	Clear.	— —	Hazy.	SW. NE	30.0
16	54	54	—	68	60	+ 8	Clear.	— —	— —	W	29.9
17	48	46	+2	61	60	+ 1	Showery.	— —	Clear.	N	29.8
18	50	49	+1	64	61	+ 3	Cloudy.	Partly clear.	— —	N	30.2
19	48	48	—	61	57	+ 4	Cloudy.	— —	Clear.	NE	30.3
20	45	48	−3	65	60	+ 5	Cloudy.	Clear.	Cloudy.	NE	30.3
21	51	49	+2	64	59	+ 5	Cloudy.	— —	— —	NE	30.3
22	47	45	+2	64½	60½	+ 4	Cloudy.	Clear.	— —	NEN	30.3
23	51	49½	+1½	61	57	+ 4	Miſty rain.	— —	Cloudy.	NE	30.3
24	52	51	+1	62	58	+ 4	Cloudy.	Clear.	— —	NE	30.4
25	40	48	−8	63	60	+ 3	Cloudy.	— —	Clear.	SW. NE	30.3
26	44½	49	−4½	68	63½	+ 4½	Cloudy.	— —	Clear.	SW. NE	30.2
27	43	52	−9	67	61	+ 6	Clear.	Partly clear.	Cloudy.	SW. NE	30.0
28	56	55	+1	70	66½	+ 3½	Partly clear.	— —	Clear.	SW. NE	29.9
29	49½	55	−5½	72	68	+ 4	Clear.	— —	— —	SW. NE	29.9
30	50	55	−5	75	69	+ 6	Clear.	— —	— —	SW. NE	29.8

TABLE

TABLE III.

Thermometers at different ſtations compared together.

1786	Night.					Day.					Morning.	After-noon.	Even-ing.	Wind.	Barometer.
	In the meadow on the ground.	In the meadow at ſix feet.	On the tower.	Diff. between the ground and ſix feet.	Diff. between the ground and the tower.	In the meadow on the ground.	In the meadow at ſix feet.	On the tower.	Diff. between the ground and ſix feet.	Diff. between the ground and the tower.					
Oct.	°	°	°	°	°	°	°	°	°	°					In.
10	52	52	52½	—	− 0½	62	62	58	—	+ 4	Cloudy.	– –	– –	S	29.5
11	38	40	48	− 2	− 10	57	58	57	− 1	—	Partly clear.	– –	Cloudy.	S	29.5
12	51	51	51½	—	− 0½	58	57	54	+ 1	+ 4	Cloudy.	– –	Rain.	SW	29.5
13	36	37	38½	− 1	− 2½	52	51	47	+ 1	+ 5	Miſty rain.	Rain.	Clear.	NW	30 0
14	25½	28	32	− 2½	− 6½	51	51	48	—	+ 3	Clear.	– –	– –	NNW	30.1
15	31	33	40	− 2	− 9	52	50	42	+ 2	+ 10	Clear.	– –	– –	NNW	30.1
16	29	31½	36	− 2½	− 7	54	53	48	+ 1	+ 6	Clear.	– –	– –	NNW	30.1
17	33	36½	39	− 3½	− 6	50	50	50	—	—	Partly clear.	– –	Clear.	N	30.2
18	31	34	34	− 3	− 3	53	53	50	—	+ 3	Clear.	– –	– –	N	30.2
19	26	29	36	− 3	− 10	50	49	47	+ 1	+ 3	Clear.	Cloudy.	Clear.	N	30.2
20	43	44	45	− 1	− 2	48	47	47	+ 1	+ 1	Cloudy.	– –	– –	W	30.3
21	43	44	45	− 1	− 2	49	49	48	—	+ 1	Cloudy.	– –	– –	W	30.3
22	45	45	45	—	—	52	52	47	—	+ 5	Cloudy.	– –	– –	E	30.2
23	45	45	45	—	—	51	51	51	—	—	Cloudy.	Miſty rain.	– –	E	30.2

TABLE IV.

Thermometers at different ſtations compared together.

1786	Night.							Day.							Greateſt difference in the night.	Wind.	Weather.	Barometer.	In the ground in the meadow.	In the ground on the hill.	Temperature of the river.
	In the meadow on the ground.	In the meadow at three feet.	In the meadow at ſix feet.	On the hill on the ground.	On the hill at ſix feet.	In the garden at nine feet.	On the tower at 220 feet.	In the meadow on the ground.	In the meadow at three feet.	In the meadow at ſix feet.	On the hill on the ground.	On the hill at ſix feet.	In the garden at nine feet.	On the tower at 220 feet.							
																		In.			
Nov. 12	23°	25½°	26°	25½°	.28½°	27°	31°	43½°	43°	43½°	43½°	42°	43°	41½°	8°	E	Clear.	29.3	42°	41°	42°
13	21½	24	25	26	29	26	30	37	36½	37½	36	38	36	35	8½	E	Clear.	29.7	42	41½	40
14	23	24½	25	22½	23½	24	26	34	33	33½	35	38	33	31½	3½	E	Clear.	29.8	42	41	37½
15	29	29	29	28½	28½	29	28½	37	37½	38	38	38	38	38	0½	E	Cloudy.	29.7	41½	39½	38½
27	35	37	38	39½	42	39	44	48	47½	48	47	47	47	46	9	S	Clear.	30.0	42	41	43
28	44½	45	45	44½	45	45	45	51	51	51	50	49	51	50	0½	S	Rain.	29.7	47	45	47
29	37½	38½	38	38	40	38	43	51	51	51½	50	48	51	51	5½	S	Rain.	29.7	44	43	44
30	39½	40	40½	39	40	39	40	49	50	50½	49	45	43	40	1½	S	Miſty rain	29.7	44	43	45
Dec. 27	4	4	7	11	15	6½	21	43	43	42	42	42	42	44	17	S	Clear.	29.5			
29	25	26	28	29	32	27	34	46	47	51	46	47	45	46	9	S	Clear.	29.7			
31	28	29	31½	30½	31	29	35	39	40½	43	41	42½	43	37	7	WSW	Clear.	29.5			
1787																					
Jan. 2	25	27	29	26	28	28½	37	38	38	38	37	36	38	37	12	E	Clear.	30.4			
4	34	35½	38½	35	36	38	37½	44	45	46	44	44	44	44	4½	SW	Clear.	30.5			
7	24½	25½	28	27	27	26½	30½	35½	34	37	44	45	43	44	6	E	Clear.	30.5			
12	21	23	26	28	30	24½	31	41	42	44	43	43	37	38	10	E	Clear.	29.8			
28	13	14	17	12	16	16	25	33	34	37½	—	—	36	—	12	—	Clear.	29.8			
Feb. 6	28	29	32	31	33	31	36	45	45	48	44	44	43	43	8	S	Clear.	29.9			

TABLE V.

Thermometers at different ſtations compared together.

	Greateſt cold in the night.							
	Horizontal thermometers.		Difference.	Vertical thermometers.			Difference on the ground and three feet above.	Diff. on the ground and ſix feet above.
1786	One inch above the ground.	9 inches above the ground.		Meadow on the ground.	Meadow three feet above the ground.	Meadow ſix feet above the ground.		
Dec. 31	27½°	29°	1½°	28°	29°	31°	1°	3°
1787								
Jan. 3	33½	34	0½	33	33	34	—	1
4	37½	38	0½	34	35	38	1	4
8	21	22	1	21	22	24	1	3
11	24½	27	2½	27½	28	31	0½	3½
12	21	23	2	21	23½	26	2½	5
13	30½	32	1½	34	34½	37	0½	3
14	22½	24	1½	23	25	28	2	5
Feb. 6	27½	29½	2	28	29	32	1	4
23	18½	20	1½	19	21	23	2	4

TABLE VI.

Thermometers at different ſtations compared together.

1784	Night.				Day.				Morning.	Afternoon.	Evening.	Wind.	Barometer.
	At Chiſlehurſt.	In the garden.	On the tower.	At the ſea-ſhore.	At Chiſlehurſt.	In the garden.	On the tower.	At the ſea-ſhore.					
Oct.	°	°	°	°	°	°	°	°					In.
22	38	43½	44	47	52	55½	54	55	Cloudy.	– –	– –	SW	30.0
23	44	43	46	46	52	56	54	56	Miſty rain.	Partly clear.	Cloudy.	W	29.7
24	31	32½	34	34	42	46½	43	41	Clear.	– –	Rain.	NW	29.4
25	24	31	30½	33	36	43	40	40	Partly clear. Hail	Clear.	Cloudy.	NW	29.7
26	29	35½	32½	36	45	44	43	45	Partly clear. Hail.	– –	Showery.	NE	29.9
27	31	40	39	40	44	47	46	46	Rain.	– –	Cloudy.	NE	30.0
28	33	42	41	42	46	47½	46	47	Rain.	– –	– –	NNE	30 0
29	33	40	42	43	46	47	48	47	Cloudy.	Rain.	Showery.	NNE	30.0
30	33	42	40	41	49	49	50	50	Cloudy.	– –	Rain.	N	29 9
31	42	45	44	44	45	50	49	47	Cloudy.	Clear.	Cloudy.	NW	29.8
Nov.													
1	40	41	42	42	49	55	51	50	Rain.	Showery.	– –	SW	29.7
2	39	42	44	44	46	43½	49	47	Clear.	– –	– –	W	29.8
3	—	32½	35½	34	49	50	48½	51	Cloudy.	Clear.	– –	NW	29.9
4	34	34	38	37	48	48½	48	51	Cloudy.	– –	– –	S	29.8
5	32	33	38	38	43	45½	45	45	Miſty rain.	– –	– –	E	29.6
6	39	42	42½	44	44	46	43	45	Miſty rain.	– –	– –	E	29.5
7	38	42	42	42	44	46	44	46	Miſty rain.	– –	– –	E	29.8
8	27	35	36	41	39	45½	42	45	Clear.	– –	– –	S	30.0
9	27	28½	32	30	44	46	45	46	Cloudy.	Rain.	– –	S	29.7
10	37	38	40	41	51	54	54	—	Cloudy.	Clear.	Rain.	SW	29.6
11	41	49	50	—	56	54	51	57	Rain.	– –	– –	SW	29.3
12	45	50	50	48	49	56	48	56	Rain.	Clear.	– –	N	29.3
13	36	38	44	41	47	54½	51	49	Cloudy.	– –	Clear.	NW	29.7
14	31	38	41	44	53	55½	54	55	Cloudy.	Rain.	– –	NW	29.8
15	37	41	41	43	52	55	54	55	Rain.	– –	– –	NW	29.7
16	38	42	43	42	47	53½	51	49	Clear.	– –	– –	SW	29.9
17	37	40	37	39	51½	51	50	50	Rain. Hail.	– –	Clear.	NW	29.7
18	—	35	43	46	47	50	49	49	Showery.	– –	– –	NW	29.7
19	27	31	32	35	—	43½	37	41	Partly clear.	Rain.	– –	NW	29.8
20	25	29½	30	33	35	40	38	39	Clear.	Rain.	– –	NW	29.9
21	22	28	30	34	36	37	37	38	Clear.	– –	– –	NNW	30.2
22	25	27	29	29	40	40½	45	43	Partly clear.	Showery.	Cloudy.	W	

TABLE VII.

Thermometers at different ſtations compared together.

1785	Night.				Day.				Morning.	Afternoon.	Evening.	Wind.	Barometer.
	At Chiſlehurſt.	In the garden.	On the tower.	At the ſea-ſhore.	At Chiſlehurſt.	In the garden.	On the tower.	At the ſea-ſhore.					
June													In.
1	46°	47°	46°	47°	61	65°	59°	61°	Showery.	– –	– –	W	29 8
2	47	47	48	50	61	65	61	62	Clear.	Cloudy.	– –	N	29.8
3	49	46	47	46	74	71	67	69	Partly clear.	– –	– –	SW	29.9
4	45	42	46	44	71	70	66	69	Partly clear.	– –	Clear.	SW.NE	29.8
5	52	54	54	54	64	68	60	66	Partly clear.	Cloudy.	Rain.	W	29.7
6	48	51	49	51	63	67	57	61	Cloudy.	– –	– –	S. N	30.0
7	43	43	45	45	63	62	58	60	Cloudy.	– –	Rain.	SES	30.0
8	53	53	52	54	67	73	66	70	Rain.	– –	Showery.	SSW	30.0
9	55	56	55	55	75	75	70	73	Partly clear.	– –	– –	SW	30.2
10	50	56	55	52	82	78	74	86	Clear.	– –	– –	W	30 3
11	54	54	57	57	78	84	74	85	Clear.	– –	– –	SW	30.4
12	56	53	57	55	76	71	63	84	Clear.	– –	– –	SW.NE	30.3
13	49	41	49	—	77	67½	64	—	Clear.	– –	– –	SW.NE	30.2
14	46	42½	50½	47	71	69	64	68	Clear.	– –	– –	SW.NE	30.2
15	51	48	53	51	79	78	73	76	Clear.	– –	Hazy.	SW.NE	30.0
16	51	54	54	50	63	68	60	71	Clear.	– –	Rain.	W	29.9
17	47	48	46	47	63	61	60	61	Showery.	– –	Clear.	N	29.8
18	48	50	49	51	66	64	61	64	Cloudy.	Partly clear.	– –	N	30.2
19	44	48	48	50	69	61	57	63	Cloudy.	– –	Clear.	NE	30.3
20	42	45	48	51	69	65	60	61	Cloudy.	Clear.	Cloudy.	NE	30.3
21	52	51	49	50	67	64	59	60	Cloudy.	– –	– –	NE	30.3
22	42	47½	44	51	70	64	60	62	Cloudy.	– –	– –	NEN	30.3
23	48	51	49	50	70	61	57	63	Miſty rain.	– –	Cloudy.	NE	30.3
24	44	52	51	54	69	62	58	60	Cloudy.	Clear.	– –	NE	30.4
25	45	40	48	53	70	63	60	65	Cloudy.	– –	Clear.	NE	30.3
26	47	44	49	52	75	68	63½	64	Cloudy.	Clear.	– –	SW.NE	30.2
27	51	43	52	51	71	67	61	63	Clear.	Partly clear.	Cloudy.	SW.NE	30.0
28	53	56	55	56	72	70	66	70	Partly clear.	– –	Clear.	SW.NE	29.9
29	50	49½	55	55	75	72	68	66	Clear.	– –	– –	SW NE	29.9
30	49	50	55	57	81	75	69	67	Clear.	– –	– –	SW.NE	29.8

TABLE VIII.

Thermometers at different ſtations compared together.

	Night.				Day.		
	In the garden.	On the tower.	At the ſea-ſhore.		In the garden.	On the tower.	At the ſea-ſhore.
1784, Dec. 10	9°	15°	14°		21°	20°	27°
11	5	12	12		25	22	29
25	17	21	21		33	32	33
26	23	27	27		36	35	32
1785, Feb. 7	25	28½	28		38	38	37
18	15	17	13	A little ſnow fell on theſe days.	30	31	30
21	10	15	13		38	36	36
22	17	21	16		32	33	38
25	27	31	31		44	41	40
Mar. 1	18	21	20		32	31	32
3	23	25	25		36	35	36
6	25	30	30		38	39	38

IX. *Obſervations on the Manner in which Glaſs is charged with the electric Fluid, and diſcharged. By* Edward Whitaker Gray, *M. D. F. R. S.*

Read January 17, 1788.

DR. FRANKLIN, in various parts of the firſt volume of his Experiments and Obſervations, aſſerts, That the natural quantity of electric fluid in glaſs cannot be increaſed or decreaſed; and that it is impoſſible to add any to one ſurface of a plate or jar, unleſs an equal quantity be, at the ſame time, given out from the other ſurface [*]. This error has been adopted by ſucceeding electricians; among others, by the late Mr. HENLY, who in one of his laſt Papers, printed in the Philoſophical Tranſactions for the year 1777, has the following words: "According to Dr. FRANKLIN's theory, "the ſame quantity of the electric matter which is thrown "upon one of the ſurfaces of glaſs, in the operation of charging it, is at the ſame time repelled or driven out from the other "ſurface; and thus one of the ſurfaces becomes charged plus, "the other minus; and that this is really the caſe is, I "think, ſatisfactorily proved, &c. [†]."

BECCARIA alſo has adopted the ſame opinion, ſaying, "That "a quantity of exceſſive fire cannot be introduced into one

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[*] "The quantity proportioned to glaſs it ſtrongly and obſtinately retains, and "will have neither more nor leſs." Experiments and Obſervations, Vol. I. p. 26. See alſo p. 75. 81. et alibi.

[†] Philoſophical Tranſactions, Vol. LXVII. p. 100.

" ſurface, but inaſmuch as an equal doſe of natural fire can " quit the other ſurface *."

Theſe aſſertions are, I apprehend, directly contrary to what really happens. Inſtead of which, I believe, we may ſafely aſſert, that glaſs, and every other known ſubſtance, may have its natural quantity of electric fluid either increaſed or diminiſhed to a certain limited degree; which degree bears no proportion to the quantity of matter contained in a body, but is (*cæteris paribus*) in proportion to the extent of its ſurface.

This law, which is, perhaps, without exception, may be conſidered as one of the fundamental laws of electricity, and one upon which many of its principal phænomena depend. At preſent I ſhall only conſider it ſo far as it is the cauſe of what is commonly called the charge of a coated jar.

Suppoſe ſuch a jar inſulated, and connected by its knob to the prime conductor of an electric machine; if then the machine be put in action, a certain quantity of electric fluid (agreeable to the above-mentioned law) is added to the natural quantity belonging to the inner ſurface of the jar. After which, if the finger, or any other conducting ſubſtance, be preſented to the outer coating of the jar, a quantity of electric fluid, nearly equal to that thrown in, comes from it. But this departure of electric fluid from the outſide of the jar, cannot be (as Dr. FRANKLIN ſuppoſes it) the cauſe which permits the addition of fluid to the inſide, but is merely the conſequence of the action of that ſuperfluous quantity which was thrown in. And the operator may, if he pleaſes, inſtead of taking electric fluid from the outſide of the jar, take out again (by touching the knob) nearly the whole of what he had thrown in, which he could not do if an equal quantity had already gone from the outſide of the jar *.

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* Treatiſe upon Artificial Electricity. Eng. Tranſ. ſect. 181.

When the quantity already ſpoken of has been taken from the outſide of the jar (the equilibrium being nearly reſtored) another quantity like the firſt may again be added to the inner ſurface: after which a ſimilar quantity may again be taken from the outſide: thus, by the ſucceſſion of a ſufficient number of the quantities allowed by the before-mentioned law, the jar may, at length, be completely charged.

There are other ways of charging coated glaſs; but if it be allowed, that the charge, in the foregoing inſtance, is produced in the manner I have ſuppoſed, it will not, I think, be diſputed, that all other charges are produced by a ſimilar alternation of ſmall quantities. This, however, will appear more clear from the obſervations I ſhall now make on the manner in which the diſcharge is produced.

When the aſtoniſhing velocity with which the charge of a jar or battery moves through a conſiderable ſpace is conſidered, it may at firſt appear impoſſible, that the diſcharge ſhould be made by the alternate giving and receiving ſuch ſmall quantities as thoſe by which the charge was produced; yet a more ample conſideration of the matter will, I think, ſhew that it cannot poſſibly be brought about any other way.

I preſume it will be granted, that the charge of a jar (in diſcharging) either leaves it all at once, or goes out by the ſame ſmall quantities by which it went in. To ſuppoſe any intermediate

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* Much diſpute has ariſen among electricians reſpecting the degree of charge which may be given to an inſulated jar; but no one, that I know of, has taken notice of a deception which will happen, if care be not taken that the ſame ſide, by which the jar is attempted to be charged, be firſt touched in trying whether it be charged or not; whereas it is clear, from what has been ſaid, that, if the contrary ſurface be firſt touched, a ſmall charge will, from that very circumſtance, be produced.

mediate manner would neither leſſen the difficulty, nor would it be conſonant to any of the known laws of electricity.

If then the whole charge leave the jar all at once, there muſt be a point of time at which the jar will be without any electric fluid either on one ſide or the other: nay more, ſuppoſe a large jar or battery to be diſcharged by means of a few inches of thin wire, there will then be a point of time at which the whole quantity of electric fluid, which conſtituted the charge, muſt be contained in a piece of wire, weighing only a few grains.

Now, if it be conſidered, that time (like matter) is infinitely diviſible, may we not rather ſuppoſe, that the diſcharge of a jar is nothing more than an inconceivably rapid ſucceſſion of ſuch ſmall quantities as may be ſent off, without cauſing ſuch a deſtruction of the equilibrium as the laws of electricity ſeem not to admit?

That this ſuppoſition is not quite free from objections I readily admit; but before they are permitted to overthrow it, let it be well conſidered, whether they are (upon the whole) as ſtrong as thoſe I have ſtated againſt the oppoſite opinion, which I think may be pronounced to militate not only againſt what has been here mentioned as a fundamental law of electricity, but alſo againſt every known fact.

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X. *Experiments on the cooling of Water below its freezing Point.* *By* Charles Blagden, *M. D. Sec. R. S. and F. A. S.*

Read January 31, 1788.

WHEN the experiments for determining the degree of cold at which quickſilver becomes ſolid, related in the Philoſophical Tranſactions for 1783, were under conſideration, no difficulty occurred in explaining the phænomena that had been obſerved, except in the few inſtances where the mercury in the thermometer congealed, whilſt it was ſurrounded with ſome of the ſame metal in a fluid ſtate. The well-known property of water, that under different circumſtances it will bear to be cooled ſeveral degrees below its freezing point without congealing, afforded from analogy the moſt probable ſolution of this difficulty; but as neither the cauſe of that property had been inveſtigated, nor the circumſtances by which it is modified had been aſcertained, I was led to attempt ſome expements on the ſubject; not only in hopes of elucidating the above-mentioned phænomenon of the quickſilver, but alſo becauſe this very quality in water was itſelf a curious ſubject of reſearch.

I began with endeavouring to determine, whether this property belongs to it as pure water, or depends upon extraneous admixtures. For that purpoſe I poured ſome clean diſtilled water into a common tumbler glaſs, till it reached two or three inches above the bottom, and then ſet the glaſs in a frigorific mixture, made with ſnow and common ſalt. This was the method I uſed in moſt of the following experiments, ſometimes employing ice inſtead of ſnow, ſubſtituting a glaſs jar or cylinder inſtead of a tumbler, and filling the veſſel to a greater or leſs height above the bottom. I found that, in the frigoric mixture, the diſtilled water readily ſunk many degrees below 32°, ſtill continuing fluid; and by repeating the experiment with care, I ſeveral times cooled it to 24°, 23°½, and even almoſt to 23°. The temperature was aſcertained by means of a ſmall thermometer with a ſliding ſcale; and though the water was not of the ſame degree of cold throughout, yet the difference, when the experiment had been well conducted, was not conſiderable; and I was particularly careful that the thermometer ſhould not touch the ſides or bottom of the glaſs, ſo as to be affected immediately by the cold of the mixture. From theſe experiments therefore it ſeemed evident, that the property of being cooled below the freezing point did not depend on extraneous admixture, eſpecially as I found, by comparative trials, that common pump-water would ſcarcely ever bear to be cooled ſo much. An ambiguity, however, ſtill remained, on account of the air which is always mixed with water that has lain expoſed to the atmoſphere. In order to determine what might be aſcribed to this circumſtance, I put ſome of the ſame diſtilled water over the fire to boil, in a clean ſilver veſſel, and kept it in violent ebullition for a conſiderable time. In a few minutes after it had been taken off the fire, and before it was nearly cold, I ſet it in the frigorific mixture after the uſual manner; when, inſtead of freezing more readily, it bore to be cooled two degrees lower than I had ever been able to reduce the unboiled water, not congealing till the thermometer in it had ſunk to 21°. Subſequent experiments were attended with a ſimilar reſult, and have ſufficiently convinced me, that, other things equal, boiled water may be cooled a greater number of degrees below the freezing point, without congealing, than water which, not having undergone that operation, retains the air it naturally imbibes.

As a further proof that the preſence of an aërial fluid in water rather leſſens than increaſes its quality of being cooled below the freezing point, I found that diſtilled water, which had been for that purpoſe impregnated with fixed air, generally ſhot into ice at a leſs degree of cold than the ſame water in its ordinary ſtate. I ſuſpect, however, that it is uſually by the admixture of other aërial ſubſtances, ſuch as dephlogiſticated air, phlogiſticated air, or perhaps both, and not of fixed air, that water is inclined to congeal ſoon after it has paſſed the freezing point; for, as will be ſeen hereafter, *acids* rather improve than diminiſh the quality in water of reſiſting congelation.

To determine the effect of other extraneous ſubſtances, I took ſome very hard pump-water, ſuch as is found in the northern parts of London, and ſet it in the frigorific mixture. In general it congealed ſooner by one or two degrees than unboiled diſtilled water; that is, at 25° or 24° of the thermometer; and as there was ſome variation in this reſpect, I was led to remark, that the greateſt cooling uſually took place when the water was moſt clear and tranſparent. With a view to this circumſtance I took ſome New-River water, which happened at that time to be conſiderably turbid, and tried it in the frigorific mixture; when I found, very unexpectedly, that it was not in my power to cool any of it below the freezing point; a cruſt of ice always forming round the ſides and at the bottom of the veſſel, whilſt the thermometer, ſuſpended about the middle of the water, was two or three degrees above 32°. To try how far this depended on the foulneſs of the water, I collected ſome of the muddy ſediment which had been depoſited from the New-River water, and added it to the pump-water, which had before born to be cooled to 24° or 25°, ſo as to render it turbid; when it congealed, in the ſame manner as the New-River water had done, before the thermometer in the middle of it came to the freezing point. It muſt not however be imagined, that water thus made turbid is incapable of being cooled below 32°, without freezing: I have ſince repeated the experiments, with more caution in conducting them, and reduced it two or three degrees below the point of congelation. But ſtill they have all confirmed the general fact, that ſubſtances which leſſen the tranſparency of water, render it at the ſame time much more difficult to be cooled below the freezing point, and diſpoſe it to ſhoot into ice more readily, after it has paſſed that point, than pure water would do. It ſeems to be of little conſequence what the ſubſtance is that renders the water turbid: ſmall particles of any kind floating through it, I believe, have this effect, which does not take place, or at leaſt to the ſame degree, when the extraneous ſubſtance has ſubſided to the bottom.

It is this circumſtance, I ſuppoſe, which gave riſe to the opinion, that boiled water freezes ſooner than unboiled: for if the water contain calcareous earth, held in ſolution by means of fixed air, as is the caſe with moſt kinds of ſpring-water, this will be precipitated by the boiling, and will ſenſibly trouble the tranſparency of the water; which, if expoſed to the cold in that ſtate, will be liable to freeze ſooner than the ſame kind of water unboiled and tranſparent *.

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* See Philoſophical Tranſactions, Vol. LXV. p. 124.

The effect of this want of tranſparency was very different from that of chemical mixture, as appeared by ſubſequent experiments.

Though the property of being cooled below the freezing point appeared to belong eſſentially to water in its pure ſtate, it was probable that it would be in ſome meaſure altered or modified by the various ſubſtances which are capable of being diſſolved in, or chemically combining with, the water. But here a further circumſtance came to be conſidered. It is well known, that ſuch ſubſtances, uniting with water, have a power of lowering its point of congelation a greater or leſs number of degrees, according to the nature and quantity of the ſubſtance employed. The firſt object, therefore, was to determine in what manner the property of bearing to be cooled would be affected with regard to that new point of congelation. For this purpoſe I made many experiments with ſeveral different ſubſtances, which it would be too long to relate in detail, but the principal were as follows.

Having diſſolved in diſtilled water as much common ſalt as lowered its freezing point to 28°, I cooled it to 18°½ before it congealed. Another ſolution of the ſame ſalt, whoſe freezing point was 16°, bore to be cooled to 9°; and a ſtronger ſolution, whoſe freezing point was 13°¼, cooled to 5° before it ſhot. A ſolution of nitre, whoſe freezing point was 27°, cooled to 16°, that is, eleven degrees below its new freezing point; a ſolution of ſal ammoniac, whoſe freezing point was 12°, cooled to 3°; and one of Rochelle ſalt, freezing point 27°¼, ſuffered the thermometer to ſink in it to 16° before it froze; a cooling equal to the greateſt I ever obtained with the pureſt diſtilled water boiled. A ſolution of green vitriol, whoſe freezing point was near 30°, cooled below 19°: and, of ſalts with an earthy baſis, a ſolution of the common bitter purging ſalt, whoſe freezing point was at 25°½, bore to be cooled to 19°.

Acids, as I have already had occaſion to remark, rather augment this quality of being cooled below the freezing point. A combination of nitrous acid with diſtilled water, in ſuch proportions that the new freezing point was between 18° and 19°, ſunk down to 6° before it congealed; which being fully 12 degrees of cooling, is greater than I have been able to produce with pure water. Another mixture of the ſame kind, ſo ſtrong as to have its freezing point about 11°, cooled down to 1°. A mixture of vitriolic acid and diſtilled water, whoſe freezing point was 24°½, cooled to 14°; and one with the acid of ſalt, having its freezing point at 25°, ſunk to 16° before it froze. It is here to be obſerved, that theſe acid mixtures were rather remarkable for the ſteadineſs with which they bore to be cooled, and the little tendency they ſhewed to ſhoot before they were ſunk much below the freezing point, than for exceeding the number of degrees which pure water might be cooled. Of the alkalies, a ſolution of tartar, whoſe freezing point was 25°½, cooled to 18°; and another, with the freezing point at 15°, ſunk to 8°. A ſolution of cryſtalliſed ſoda, freezing point 30°, cooled to 21°; and a ſolution of mild volatile alkali, freezing point 19°, to 11°. A mixture of rectified ſpirit of wine and water, whoſe freezing point was 12°, cooled to 5°; and another, with the freezing point at 8°½, to 2°.

All theſe facts, with many others of the ſame nature which I obſerved, ſufficiently ſhew, that foreign ſubſtances, chemically combined with or diſſolved in water, do not take away its property of being cooled below its point of congelation; though, by depreſſing that point, they alter the degree of cold at which the property commences. The experiments ſhew, that in ſome caſes the mixed water bore to be cooled as much below its new freezing point, as pure water below 32°; and with regard to the others, I think the variation was no greater than uſually takes place with different portions of common water. Scarcely any, perhaps none, of the above-mentioned points, were abſolutely the loweſt to which the ſolutions or mixtures could have been reduced, if the experiment had been conducted ſtill more ſlowly and cautiouſly. But however much they might all poſſibly have born to be cooled, a great difference occurred among them in the eaſe with which the operation ſucceeded. For inſtance, the ſolutions of nitre, and of Rochelle ſalt, would hardly ever ſhoot till they were cooled many degrees below their reſpective freezing points, however negligently the operation was conducted; whereas thoſe of common ſalt, ſalt of tartar, and ſome others, required conſtant attention to keep them from freezing, as ſoon as they were got four or five degrees below the point of congelation. Their difference in this reſpect may depend in part upon ſomething unknown in the nature of each particular ſalt; but there was one circumſtance to be diſtinctly traced, correſponding to what had been obſerved with pure water; namely, that the moſt tranſparent, moſt limpid ſolutions, were thoſe which admitted of being cooled with the greateſt eaſe and certainty. The ſame obſervation holds good with regard to the mixtures: thus the rectified ſpirit I employed aſſumed ſomething of an opaline tinge upon being mixed with the water (from the ſeparation of an oil it contained) and the compoſition bore to be cooled but ill, though a film of oil, regularly ſpread on its ſurface, would rather have retarded the freezing; and the acid mixtures, which cooled ſo remarkably well, ſtruck the eye particularly on account of their very perfect and uniform tranſparency. In this laſt caſe, perhaps, another circumſtance might contribute to the eaſy cooling, that the acids, by combining with the water, ſeemed to expel the air it contained more perfectly than moſt other ſubſtances, as appeared from the innumerable ſmall bubbles that were almoſt immediately formed.

Want of tranſparency, however, is only one among ſeveral cauſes which impair the property water naturally poſſeſſes, of bearing to be cooled many degrees below its freezing point. M. MAIRAN, in his elaborate Treatiſe upon Ice, having occaſion to examine this ſubject, was led by his experiments to conclude, that the cooling of water below its freezing point depends upon reſt, and that agitation is the general cauſe by which it is brought to ſhoot into ice. In this opinion he has been almoſt implicitly followed by all the writers I have ſeen, excepting only Profeſſor WILCKE, of Stockholm[*]. To bring it to the teſt of experiment, I ſet in the frigorific mixture ſome diſtilled water, which by boiling had been rendered capable of ſuſtaining a cold of 21° before it froze. When this water was cooled to 22°, I agitated it, by moving the tumbler, by ſhaking a quill in it, and by blowing on it ſo as to ruffle the ſurface; but it ſupported all theſe trials without congealing, and did not ſhoot till a minute or two afterwards, when by continuance in the frigorific mixture it was cooled down to 21°. In other experiments, however, all the above-mentioned kinds of agitation made ſimilar water inſtantly congeal, even when not cooled ſo low by ſeveral degrees. The congelation, therefore, muſt in theſe caſes have depended on ſome further circumſtance than the mere want of reſt. One that I ſuſpected is a ſort of tremulation, rather agitating ſmall portions of the water ſeparately, than moving the whole together. I have found, that ſtriking the bottom of the tumbler againſt a board would produce inſtant congelation, when ſtirring the water, or ſhaking the tumbler in the hand, would have no effect. In like manner, when in ſtirring the cooled water the quill, or ſtick of glaſs, employed for that purpoſe, ſtrikes againſt the ſide or bottom of the tumbler, the water, which had reſiſted the general ſtirring, is often by this percuſſion made to freeze. The ſame effect is produced, and with leſs uncertainty, if the quill, or ſtick of glaſs, be rubbed, and as it were ground, againſt the ſide of the tumbler. But of all ſuch methods of bringing on the congelation, that which I have found to fail the ſeldomeſt, is to rub a bit of wax againſt the ſide of the tumbler under the water; a particular roughneſs in the motion is felt, with ſome ſound, approaching to a muſical tremulation, and a cruſt of ice is immediately perceived under the wax upon the glaſs. This effect of the wax I take to be mechanical, depending on its particular ſtate of conſiſtence. Wood acts in the ſame manner, though with leſs certainty; ſo does a quill, and likewiſe glaſs; but the latter, being very hard, produces the effect with leaſt certainty. It is a mechanical action upon the water in contact with the rubbing ſubſtance and the glaſs: for if the outſide of the tumbler, or any part of the inſide above the water, be rubbed, even if it be wet ſo as to communicate a ſimilar feeling of tremulation, yet ſtill the congelation is not produced.

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[*] Kongl. Vetenſk. Acad. Handlingar, Vol. XXX. p. 103. 105.

All theſe modes of bringing on the congelation ſucceed beſt, as might be expected, in proportion as the water is more cooled below the freezing point. Unleſs the cooling amount to four or five degrees, the friction with wax is often in vain.

From the above-mentioned facts it appears, that M. MAIRAN's poſition, though not deſtitute of foundation, was enounced by him too generally, and without ſufficient preciſion. It is the natural property of water to bear to be cooled a certain number of degrees without freezing; reſt favours this property negatively, by giving it no interruption; but moſt kinds of agitation interfere with its operation to a greater or leſs degree, and ſome perhaps would prevent it altogether; whilſt others affect it ſo little, as not to ſuperinduce the congelation, even when the cooling is brought within one degree of the greateſt that the water will bear.

Whatever be the effect of agitation, there is another cauſe which much more powerfully haſtens the congelation of water. It has been long known, that when water is cooled below its freezing point, the contact of the leaſt particle of ice will inſtantly make it congeal, the glacial cryſtals ſhooting all through the liquor, from the ſpot where the ice touches it, till the whole comes up to the freezing point. Few experiments of the minute kind afford a more ſtriking ſpectacle than this, eſpecially when the water has been cooled nearly as much as poſſible below the freezing point; both from the beautiful manner in which the cryſtals ſhoot through it, and the rapidity with which the mercury in the thermometer immerſed in it runs up through a ſpace of 10 or 11 degrees, ſtopping and fixing always at 32 in pure water. If from any circumſtance, however, as a leſs cooling, or the addition of a ſalt, the ſhooting of the ice proceed more ſlowly, the thermometer will often remain below the freezing point even after there is much ice in the liquor; and does not riſe rapidly, or to its due height, till ſome of the ice is formed cloſe to its bulb; which exemplifies the evolution of the latent heat from the very particles that congeal.

Many of the circumſtances attending the greater or leſs cooling of water below its freezing point depend upon this principle. In a calm day, when the temperature of the air was about 20°, I expoſed two veſſels with diſtilled water to the cold; one of them was ſlightly covered with paper, the other was left open: the former bore to be cooled many degrees below the freezing point, whilſt a cruſt of ice always formed on the ſurface of the other before the thermometer immerſed in the middle of it came to the freezing point. This phænomenon, which other obſervers have remarked without being able to account for it, appears to me clearly owing to frozen particles, which in froſty weather are almoſt always floating about in the air, often perceptibly to the ſenſes. They come moſt commonly either from clouds paſſing over head, or from ſnow or hoar-froſt lying upon the earth; and when they touch the cooled ſurface of the water, inſtantly make it freeze. That the effect does not depend ſimply on the contact of cold air, is plain from the following experiment. I expoſed to the cold a glaſs jar, with ſome diſtilled water, and placed in it two thermometers; one immerſed in the water, the other ſuſpended a little above its ſurface, in the empty part of the jar. The latter ſunk faſter than the former; but after a certain time, the thermometer above the ſurface was at 25°, and that in the water at 25°½, yet the water continued unfrozen. I perceive too by M. WILCKE's experiments, that in much more intenſe cold than we uſually experience in this country, veſſels of water ſtanding within doors in a laboratory are often cooled ſo far below the freezing point as to become almoſt full of ice upon being made to ſhoot, though the ſurface of the water be in no wiſe defended from the cold air of the laboratory. Oil ſpread over the ſurface of water has been found to prevent it from freezing, when other water ſimilarly expoſed has had a cruſt of ice formed upon it. This I aſcribe entirely to the prevention of frozen particles from coming in contact with the water: for in experiments with frigorific mixtures, in a room of moderate temperature, I do not find that oil on the ſurface has any ſenſible effect in enabling water to ſupport more cold, unleſs, indeed, where the operation is otherwiſe too much precipitated. Alſo a crack in the tumbler containing the water prevents it from cooling below the point of congelation, a thin film of ice inſinuating itſelf through the crack into contact with the water. And often, in experiments with frigorific mixtures, the congelation is brought on by raiſing the immerſed thermometer a little out of the water, and lowering it down again; ſome of the adhering water having frozen upon its ſtem.

Several other circumſtances, though not ſo diſtinctly aſcertained as the preceding, appear to facilitate the congelation of cooled water. For inſtance, in experiments with frigorific mixtures, if the cold be very intenſe, the water freezes almoſt immediately round the ſides of the veſſel, as if ſomething depended on too ſudden a change of temperature. Accordingly, the only way of inſuring the greateſt degree of cold in water without freezing, is to cool it in a very gradual manner, keeping the cold of the frigorific mixture regularly only two or three degrees below that of the water. Sudden cooling, therefore, may be conſidered as one of the cauſes which haſten congelation. No doubt this will ſometimes depend on ſuch a cold as water cannot reſiſt without freezing, being propagated through the glaſs to the neareſt part of the water, quicker than it can be diſtributed to the reſt of the water; but, I think, the above-mentioned effect takes place when no part of the fluid can be ſuppoſed to be many degrees below the freezing point.

It has been alledged, that metal in contact, either with the outſide of the veſſel containing the water, or with the water itſelf, diſpoſes it to freeze ſooner after it is cooled below 32°. Though upon repeating this experiment I have found it poſſible to cool water in a metal veſſel many degrees below its freezing point, and even to touch it, when ſo cooled, with metal equally cold, without producing congelation; yet the metal certainly tends to haſten the freezing, and, I believe, on the above-mentioned principle of too quick a change of temperature, occaſioned by its quality as a good conductor of heat. For the ſame reaſon it is more difficult to cool water much below the freezing point in thin veſſels than in thoſe whoſe bottom and ſides are of conſiderable thickneſs; the latter tranſmitting the heat more ſlowly, and allowing it thereby to be diffuſed more equably.

In cooling water below its freezing point by frigorific mixtures, it is of conſequence to keep the mixture ſome way below the upper edge of the water within the tumbler, otherwiſe the congelation quickly begins at that place. This very likely depends on the principle laſt mentioned, that the thin edge of water riſing up againſt the ſide of the glaſs, being more in contact with air than with the general maſs of water, does not ſo eaſily diſtribute its cold, and therefore ſuffers a more rapid change of temperature by the action of the mixture. Hence one of the moſt eſſential precautions for cooling water to the utmoſt without congelation, is to perform the experiment in a warm room, that the air in contact with the edges and ſurface of the water may prevent their ſudden cooling. And one of the moſt convenient veſſels for the purpoſe is a round body terminating in a neck, the body to be ſurrounded with the frigorific mixture, whilſt the water in the neck is kept above the freezing point.

Theſe are the principal facts with which my experiments have furniſhed me relative to the cooling of water below its point of congelation. I ſee no general circumſtance that applies to them all. At one time I thought that much depended on reducing the water into thin plates; an idea which was principally ſuggeſted by the more ready congelation of the edges of the water where it riſes up as a thin film on the ſides, and by the effect of extraneous ſubſtances floating in the water to haſten its freezing, which might be ſuppoſed frequently in their motions to intercept ſmall portions of the fluid, and form it into thin plates. Agitation likewiſe might act by reducing the water in ſome part or another into ſimilar plates. And as water impregnated with air appeared leſs capable of being cooled than the ſame water deprived of its air, it ſeemed not impoſſible that the air might act likewiſe by producing thin plates in different parts of the fluid. With a view to this hypotheſis I made ſeveral experiments. Into a tumbler with diſtilled water I put a quantity of ſand, which ſettled looſely to the bottom, and left the water above as tranſparent as before. This tumbler being placed in the frigorific mixture, the water bore to be cooled as well as it had done without this addition. Laying thin bits of glaſs upon one another at the bottom of the tumbler, in place of the ſand, I found no difference in the effect. By this latter experiment it was moreover proved, that points in the water do not perceptibly facilitate its cryſtalliſation into ice. Now the thin plates or wedges of water, which muſt be intercepted by the bits of ſand or glaſs, ſeem very analogous to ſuch as may be produced by floating particles of extraneous matter; and therefore the effect of the latter is probably to be aſcribed to ſome other cauſe, ſuch as haſtening the cooling, or rendering it leſs uniform, or, perhaps, communicating motion to the fluid.

In the preceding inſtances, however, the water may be conſidered as conſtituting, by its adheſion, a ſort of continued body with the ſubſtance it touches; and it might be ſuppoſed that, if one or both ſurfaces of the thin plate were in contact with air only, the congelation would take place. I therefore put down ſome air, retained by a ſmall diſh of wax, into the water to be cooled, and let it ſtay there till the whole was reduced many degrees below the freezing point; but no congelation enſued: I then contrived to turn up the diſh of wax in the water, ſo that the air it had carried down, eſcaping from under it, aſcended as a bubble to the top of the water, and there burſt: ſtill the water retained its fluidity, notwithſtanding the contact of a looſe bubble of ſuch cold air, and the motion produced by its exploſion on the ſurface. In other experiments, I have indeed ſeen the burſting of a bubble on the ſurface apparently bring on the congelation; but ſeemingly from the agitation it occaſioned. And though a wetted ſurface, or a ſmall thin portion of water, freeze in general more readily than a larger maſs, yet I have ſeen inſtances of a jar cooled many degrees below the freezing point, and the water and air in it equally cold, which had, notwithſtanding, upon its inſide, from the ſurface of the water to an inch or more above it, an evident dew that remained quite fluid, and rendered the ſurface of the glaſs wet. Under the ſame circumſtances alſo, I have raiſed the thermometer out of the cooled water, without occaſioning any congelation in the adhering thin film of that fluid. Theſe facts, though they do not ſhew but that a film of water, in contact on both ſides with air below its freezing point, would at once congeal, yet, I think, are ſufficient to prove, that its formation into thin plates is not a general cauſe of its early freezing.

From a conſideration of the above-mentioned cauſes of freezing, and their various exceptions, I am led to think, that the matter in queſtion depends upon ſome circumſtance of the more intimate nature or compoſition of the water; for inſtance, the arrangement, attractions, and perhaps ſhape, of its particles. If we ſuppoſe the particles of water to poſſeſs a kind of polarity, that is, to have particular attracting points or ſurfaces, properly arranged, not only its cryſtalliſation in regular angles, but likewiſe moſt of the above-mentioned phænomena, admit of ſome kind of explanation. For latent heat, be it a matter or motion, may be conſidered as a cauſe either leſſening the power or impeding the operation of this polarity; the effect of which is gradually diminiſhed by external cold, till at length the polarity entirely overcomes the reſiſtance it occaſions, and the attracting points or ſurfaces ruſh together. Whatever tends, therefore, to bring theſe particles into a ſtate more advantageous for their junction, as by preſenting their attracting ſurfaces more directly to one another, forcing them nearer together, or removing attractions of a contrary tendency, and allowing the particles free ſcope to follow the impulſe of their polarity, muſt tend to haſten the congelation. When particles of water, already frozen, are preſented to other fluid water of a proper degree of coldneſs, not only the attracting ſurfaces will be in the moſt favourable poſition from the arrangement they have taken in the freezing, but very poſſibly their power may be ſtronger from their union with one another; and hence ſuch water is inſtantly brought to freeze. Indeed it is this circumſtance which conſtitutes the freezing point; for that point is evidently nothing elſe but the degree of cold which renders the partieles of any fluid incapable of reſiſting the attractive power of other particles of the ſame fluid, already reduced into a ſolid form: and the fact, that the cold of the freezing point is leſs than the fluid will otherwiſe bear, ſeems a proof that when the particles have acquired this arrangement of ſolidity, their attractive powers are the ſtrongeſt; ſo that the difference between the freezing point, and the greateſt cold the fluid will bear, may be conſidered as the meaſure of this additional attractive force.

Agitation may be eaſily conceived, by the various motions impreſſed upon the particles, to occaſion ſome of them to apply their polar points in a more advantageous poſition, or even to force them nearer together; and theſe effects are more likely to be produced by an intimate agitation, than by a general motion of the whole maſs. The want of tranſparency, in certain caſes, as in ſome ſolutions of ſalts, ſeems not owing to the preſence of foreign matters, but rather to depend upon a particular arrangement of the combined particles, which may diſpoſe thoſe of the water to join more readily, and detach thoſe of the ſalt. Extraneous ſubſtances, beſides their indirect effect, may, by various chances attending their floating in the water, throw the particles into favourable ſituations; and if thin plates are more diſpoſed to freeze, it may be, that the particles of water in ſuch are more free from counteracting attractions.

Sudden cooling may promote congelation ſimply by occaſioning the water at the bottom and ſides of the veſſel to acquire a greater degree of cold than the reſt. But perhaps it may have alſo another effect, admitting of a particular explanation.

nation. Water in freezing undergoes a conſiderable expanſion. This may be aſcribed to ſuch a form of its particles, and poſition of their poles, as ſhall make them, when touching and adhering by thoſe poles alone, intercept very large interſtices, which may be conſidered as the *pores* of the ice. Various poſitions of the poles and figures of the particles may be conceived, which ſhould cauſe them to occupy more ſpace, when touching in certain points only, than they filled when lying near without any contact. But in whatever way the expanſion is produced, experiment hath ſhewn that it begins ſome time before congelation; ſo that when water is cooled down to 32°, it is already ſenſibly expanded; and if the congelation does not take place here, this expanſion augments, in proportion as the water is further cooled *. The expanſion, therefore, being ſo evidently an approach to freezing, may be conſidered as an indication that the polarity already prevails ſo far as to draw the particles ſomewhat out of the ſituation they naturally aſſume in the higher temperatures. And it is conceivable, that if this operation go on very quick, and the conſequent change of poſition in the particles be made with ſome degree of velocity, they may acquire a ſmall momentum of motion, enabling them to overcome a reſiſtance which would otherwiſe prevent their junction.

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* In experiments where the water has cooled much below its freezing point, I have ſeen the expanſion ſo great as to bear a conſiderable proportion to the whole expanſion produced by freezing, which laſt, I believe, is more than one-ſeventh of the volume of the water. It ſeemed to me as if the expanſion proceeded in an increaſing ratio, being much greater upon the laſt degrees of cooling than it was upon the firſt. The difficulty of procuring a proper apparatus for theſe experiments has hitherto prevented me from aſcertaining the quantities with preciſion.

As chemical combinations all depend upon attractions between the ſubſtances which unite, it is not difficult to conceive, that a particle of ſalt, an acid, or the like, may attract a particle of water in ſuch a manner as ſhall oppoſe or diminiſh its attraction for the other particles of water. Hence the polarity may be ſo much weakened, as not to bear the ſame proportion to the reſiſting power of the latent heat, till this alſo is diminiſhed by a greater degree of cold, which conſtitutes the new freezing point. But when, by increaſing the cold, all the powers are reduced to a ſimilar ſtate of equilibrium, exactly the ſame phænomena take place as belong to the natural freezing point of the water.

To aſſiſt the conception, I have here reaſoned upon the particles of water as ſolid, and of a determinate ſhape. But it ſeems moſt probable, that the particles of matter in general are nothing more than centres to certain attractive and repulſive powers; on which hypotheſis it may be underſtood, that if two or more of theſe central points are brought much within the limits of their reſpective attractions and repulſions, theſe powers will no longer be equal at equal diſtances from their common centre. Now ſuch a combination of central points may be conſidered as one particle of any particular matter; and the unequal diſtances from the common centre at which the attractions and repulſions are equal will define what may be called the ſhape of that particle. And if, at equal diſtances, the attraction or repulſion is much greater at one point than at another, that will conſtitute a polarity.

The greateſt cold I have been able to make water acquire without freezing, is near 12 degrees of FAHRENHEIT's ſcale below its common freezing point. Some diſtilled water was boiled about a quarter of an hour in a tin cup, and placed in

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the ſame veſſel, whilſt ſtill warm, in the frigorific mixture. The mixture was made to act very ſlowly, ſo that the operation continued more than an hour. When the immerſed thermometer had ſunk to 20°⅓, the water was ſtill fluid: I then ſhook it conſiderably, but no ice formed. After waiting ſome time, and finding the thermometer would ſink no lower, becauſe by the length of the proceſs the ſnow of the mixture was almoſt conſumed, I added ſome freſh materials, which could not be done without ſhaking the tin cup. Still, however, the water did not freeze inſtantly, though it ſhot as ſoon after as it can be ſuppoſed to have felt the influence of the new frigorific mixture. When this water was cooled to 24°, I tried the temperature of the air near its ſurface, and found it 34° or 35°, the experiment being performed in a room with a fire.

Another time I cooled ſome diſtilled water, covered with oil, below 21°, by ſimilar precautions.

This, however, is by no means the greateſt cooling of which water is ſuſceptible. In Fahrenheit's experiment, with an exhauſted globe half full of boiled rain water, it ſeems to have been cooled to 15°*. M. de Luc likewiſe informs us, † that having filled a thermometer with ſome water he had purged of air by the means deſcribed in his great work upon the atmoſphere, he expoſed it to a cold which ſunk a mercurial thermometer to 14° of Fahrenheit's ſcale. The water in the thermometer continued tranſparent, and upon breaking the ball was found to be liquid, but froze that inſtant. In ſome of my experiments too with mixtures of nitrous acid and water, the liquor bore to be cooled as much as 13 degrees below its new freezing point; and it has been already obſerved, that the addition of an acid always expelled much air from the water. It is not improbable, therefore, that if water could be thoroughly purged of air, it would readily bear to be cooled 18 degrees, or more, below its freezing point, without congelation; though the deprivation of air, obtained by boiling it, is ſuch only as will barely enable it to admit a cooling of 12 degrees.

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* Philoſophical Tranſactions, Vol. XXXIII. p. 81.

† Idées ſur la Météorologie, Tom. II. p. 105.

Other fluids may bear to be cooled much more below their proper point of conſolidation. This is evidently the caſe in what Mr. CAVENDISH calls * the ſpirituous congelation of acids. Mr. M^c NAB's nitrous acid bore to be cooled from 30 to near 40 degrees below its freezing point †; and Mr. KEIR's vitriolic acid at the ſtrength of eaſieſt freezing continued fluid at 29°, though its heat became 46°½ when it began to congeal ‡. How low quickſilver may be cooled has not yet been aſcertained, but probably many degrees below −40°.

So many of the above-mentioned facts were obſerved in the year 1783, that I then ventured to remark, that "independently of theſe circumſtances, neither ſtirring, agitation, a current of freſh air on the ſurface, nor the contact of any extraneous body not colder, would [neceſſarily] cauſe the water to ſhoot into ice, notwithſtanding the repeated aſſertions of authors to the contrary **." Similar experiments, made in the courſe of the ſucceeding winters, have confirmed in general the former reſults, and furniſhed the materials of the preceding ſheets. I am very ſenſible, that the ſubject ſtill remains involved in great obſcurity; nor ſhould I have troubled the Society with an account of experiments which leave ſo much uncertainty; had I not thought that they tended to elucidate a few points, and to correct ſome erroneous opinions. I hope that perſons inhabiting a climate more advantageous for the purpoſe, will be induced to undertake ſuch experiments in another, and probably a more ſucceſsful way, by expoſure to *natural* cold.

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* Philoſophical Tranſactions, Vol. LXXVI. p. 261.

† Ibid. p. 252.

‡ Ibid. Vol. LXXVII. p. 279.

** Ibid. Vol. LXXIII. p. 358.

XI. *Experiments and Obſervations relating to the Principle of Acidity, the Compoſition of Water, and Phlogiſton.* By Joſeph Prieſtley, *LL.D. F. R. S.*

Read February 7, 1788.

THAT water conſiſts of two kinds of air, dephlogiſticated and inflammable, is now, I believe, generally admitted as one of the moſt important, and beſt aſcertained, doctrines in chemiſtry. My own experiments having ſeemed to favour it, I made no difficulty of receiving it myſelf: but having, at the time of the publication of the laſt Volume of my Experiments, found that, in decompoſing the two kinds of air above mentioned by the electric ſpark, I got much leſs water than I expected, and, inſtead of it, a dark-coloured vapour, not eaſily condenſed, I could not help concluding that ſomething yet remained to be inveſtigated with reſpect to this ſubject, and determined, at a proper opportunity, to reſume my inquiries into it.

At that time, however, I had no ſuſpicion of any *acid* being produced in the proceſs; having never been able to find any in the water which I had hitherto procured in pretty large quantities from the decompoſition of thoſe two kinds of air, though the doctrine of dephlogiſticated air being, or containing, the principle of *univerſal acidity*, had been advanced by M. Lavoisier, and admitted by myſelf and others.

Suſpecting that much of the water which had been procured in the above-mentioned proceſs was no proper conſtituent part of the air, but only ſuch as had been diffuſed through it, and in ſome manner attached to it, and kept ſuſpended in it, and therefore might be ſeparated from it, without decompoſing the air; on reſuming theſe experiments, I uſed every precaution I could think of to detach all water from the air on which I operated. In order to this, I kept it confined by mercury, together with a quantity of *fixed ammoniac*, which imbibes water more readily, if not in greater quantity, than quicklime, or any other known ſubſtance.

In this more accurate method of making the experiment, I was gradually led to diſcover the acid, which had eſcaped my obſervation before. But I am not certain that I ſhould have found it even now, if I had not been aided by the ſagacity of Mr. Keir, who was always of opinion, that ſome acid *muſt* be the produce of this experiment, or rather that the produce would be ſomething which would become acid by expoſure to the open air.

I began with making the exploſions in the ſame glaſs veſſel from which the mixture of air had diſplaced the mercury with which it had been filled; when I found, as I have obſerved in my laſt publication, the whole of the veſſel was filled with a denſe ſmoke, which ſettled into a black coating of all the inſide of the veſſel, and which appeared, as before, to be mercury; becoming white by expoſure to the air. For ſome time I perceived no appearance of *water:* but placing the veſſel at a proper diſtance from a fire, I found about a quarter of a grain collected on the oppoſite ſide; when, as the veſſel contained four ounce meaſures of air, the water produced ought to have been at leaſt a grain.

The mercury being an impediment in this proceſs, I afterwards confined the mixture of air in one veſſel (with mercury and fixed ammoniac as before) but I made the exploſions in another, which I had previouſly exhauſted of air. This veſſel was larger than that which I had uſed before, containing ſomething more than eight ounce meaſures; ſo that the air it contained, being one-third dephlogiſticated and two-thirds inflammable, would have weighed about two grains. After one exploſion the quantity of water collected appearing inconſiderable, I repeated the proceſs in the ſame veſſel, and then collecting the water, I found it not to exceed a grain and an half.

I repeated this experiment very often, and conſtantly found ſome water, but it always fell far ſhort of the weight of the air decompoſed. There muſt, therefore, have been ſomething not very fluid adhering to the ſides of the veſſel, which could not be diſlodged by a moderate heat; and indeed the glaſs did not recover the perfect clearneſs that it had before the proceſs.

I always obſerved, that, preſently after every exploſion, the veſſel was filled with a denſe vapour, ſo that it was ſometimes impoſſible to ſee through it; and before I admitted the external air, I could pour it from one end of the veſſel to the other, and it ſeemed to fall almoſt as faſt as a feather in a common vacuum, and in general it did not diſappear in leſs than ten minutes. I even found this denſe vapour when the mixture of air had been confined by water. The ſmell of the veſſel, after the proceſs, was that of the moſt offenſive kind of inflammable air from iron.

From theſe experiments it was ſufficiently evident, that ſomething more than *water* had been produced; and pouring into the veſſel a quantity of the juice of litmus, it was inſtantly turned to a deep red; ſo that it was equally evident, that an *acid* had been formed. In all the preceding experiments the dephlogiſticated air had been procured from manganeſe; and in all the experiments mentioned in this Paper, the inflammable air was from iron by water only.

A great number of ſtrong glaſs veſſels having been broken in theſe experiments, and ſometimes with ſome hazard to myſelf, and the quantity of air that I was able to decompoſe in them being ſmall, I next procured a *copper* veſſel, which contained about thirty-ſix ounce meaſures of air; and having now no other object than diſcovering the *kind* of acid that I had procured, I made repeated experiments in it; and after every ten or twelve exploſions collected all the liquid matter I could find; which, as the air had been previouſly confined by water, was pretty conſiderable, about equal to the weight of the air.

The liquor that I procured in this manner was always of a deep blue or green, being evidently a ſolution of copper. But it alſo contained a redundant acid, as appeared by its turning the juice of litmus red. Beſides this blue liquor, there was always a quantity of ſeemingly abraded copper; for it was perfectly and quickly diſſolved by volatile alkali, as copper very minutely divided would have been.

In theſe experiments I uſed, at different times, dephlogiſticated air from manganeſe, from red precipitate, and from red lead, as the moſt unexceptionable of all; and as it was obligingly furniſhed me by Mr. KEIR, the preparation of it may be depended upon. There did not, however, appear to be any other difference in the liquors produced by means of theſe kinds of dephlogiſticated air, except in the ſhade of the colour; that from manganeſe being of the deepeſt blue, and that from red lead the lighteſt; and this difference might be accidental.

By the aſſiſtance of Mr. Keir I examined theſe ſolutions of copper, and preſently found, by means of a ſolution of terra ponderoſa in ſpirit of ſalt, that it was not, in any of the caſes, the vitriolic; and yet, as the dry ſubſtance left by the evaporation of the liquor did not deliqueſce, I had concluded, that the acid was neither the nitrous, nor the marine; but Mr. Keir informs me, that this is the caſe with a fully ſaturated ſolution of copper in ſpirit of nitre.

Alſo Dr. Withering, who was ſo obliging as to examine ſome of theſe liquors for me (for, not being much accuſtomed to theſe analyſes, I had requeſted him to undertake it) had procured from that in the production of which the *red lead* had been uſed, cryſtals of nitre, and other indiſputable indications of nitrous acid; ſo that I was ſatisfied that it was this acid that was produced in all the caſes.

I had a farther proof of the acid being the nitrous, that having (in order to get a quantity of liquor that ſhould be as little ſaturated with any metal as poſſible) uſed a veſſel of *tinned iron*, I found, that after ſome time, when the tin had been much corroded (and with every proceſs a conſiderable quantity came away) the liquor, which at firſt was colourleſs, was tinged with red. In theſe experiments I made uſe of dephlogiſticated air from red lead.

As both the kinds of air made uſe of in theſe experiments were exceedingly pure, it ſeems evident, that phlogiſticated air does not contain all the elements of nitrous acid; but only ſupplies a *baſe* for it, the dephlogiſticated air (which was uſed in a greater proportion in the valuable experiment of Mr. Cavendish) ſupplying the acidifying principle, as I had conjectured in the laſt Volume of my Experiments, p. 404. Beſides, though all phlogiſticated air could not be excluded in thoſe experiments in which the air-pump was uſed, this objection cannot well be made to thoſe in which that inſtrument was not uſed; and in them the ſlowly condenſable vapour above mentioned ſeems to be an evident ſymptom that the produce was not mere water. But it is a ſatisfactory anſwer to this objection, from the preſence of *phlogiſticated air* in the tube, that this kind of air is not decompoſed, or at all affected, by this proceſs, as will be found by mixing any quantity of it with the two other kinds of air.

That a conſiderable quantity of water enters into the compoſition of dephlogiſticated air, will not be thought improbable, when it is conſidered that, in my former experiments, this appeared to be the caſe with reſpect to *inflammable air*. For without water this air cannot be procured. I can alſo now ſay, that the ſame is the caſe with reſpect to *fixed air*. It is not therefore improbable, that the ſame may be true of every other kind of air, ſince water is uſed in the production of them all.

Terra ponderoſa aerata (a ſubſtance of which Dr. WITHERING has given us an excellent analyſis) gives no fixed air by mere heat. But I find, that when ſteam is ſent over it, in a red heat, in an earthen tube, fixed air is produced with the greateſt rapidity, and in the ſame quantity as when it is diſſolved in ſpirit of ſalt: and, making the experiment with the greateſt care, I find, that fixed air conſiſts of about half its weight of water.

From two ounces of the terra ponderoſa I got, by means of ſteam, 190 ounce meaſures of fixed air, ſo pure that at firſt 150 ounce meaſures of it were reduced by agitation in water to 3½, and of the laſt produce, 30 ounce meaſures were reduced to one. Examining the reſiduum of the firſt portion by means of nitrous air, I found it to be of the ſtandard of 1.5.

After this, attending to the *water* expended in the proceſs, I found that I procured 330 ounce meaſures of fixed air with the loſs of 160 grains of water. According to this, as the air weighed 294 grains, the water in the fixed air muſt have been 80 parts of 147 of the whole.

In another experiment, having previouſly found that three ounces of the terra ponderoſa yielded about 250 ounce meaſures of fixed air, I attended only to the loſs of water in procuring it, and I found it to be about one-fifth of an ounce, in two ſucceſſive trials. The quantity of fixed air would weigh 225 grains, and the water expended about 100 grains; ſo that, in this experiment alſo, the fixed air muſt have contained about one-half of its weight of water.

That water enters into the compoſition of fixed air, and adds conſiderably to its weight, is farther probable from the ſolution of terra ponderoſa in ſpirit of ſalt. Becauſe when the ſolution is evaporated to dryneſs, and the reſiduum expoſed to a red heat, the weight of the air, and of this reſiduum, exceeds that of the ſubſtance from which it was procured; and it is probable, that a red heat would expel any marine acid adhering to it.

Forty-eight grains of terra ponderoſa diſſolved in ſpirit of ſalt, and then evaporated to dryneſs, and expoſed to a red heat, loſt four grains, and yielded eight ounce meaſures of fixed air, which would weigh 7.2 grains; conſequently, three-ſevenths of the weight of the air was ſomething that had been gained in the proceſs, and therefore probably water.

The near coincidence of the reſults of theſe different experiments is remarkable, and makes it almoſt certain, that no marine acid is retained in the terra ponderoſa that has been diſſolved in it, after expoſure to a red heat; that the generation of the fixed air carries off part of the water in the menſtruum; and that this part of the weight is about one-half of the whole.

I muſt obſerve, that the ſuppoſition of water entering into the conſtitution of all the kinds of air, and being, as it were, their proper *baſis*, that without which no aëriform ſubſtance can ſubſiſt (which the preceding experiments render in a high degree probable) makes it unneceſſary to ſuppoſe, as myſelf as well as others have done, that water conſiſts of dephlogiſticated air and inflammable air, or that it has ever been either compoſed or decompoſed in any of our proceſſes.

That water is decompoſed when inflammable air is procured from iron by ſteam, is not probable; ſince the inflammable principle may very well be ſuppoſed to come from the iron, and the addition of weight acquired by the iron may be aſcribed to the *water* which has diſplaced it. Alſo when the *ſcale of iron*, or *finery cinder*, is heated in inflammable air, it gives out what it had gained, *viz.* the water.

The moſt plauſible objection to this hypotheſis is, that iron gains the ſame addition of weight, and becomes the ſame thing, whether it be heated in contact with ſteam, or ſurrounded by dephlogiſticated air. But from the preceding experiments it appears, that by far the greateſt part of the weight of dephlogiſticated air is water; and the ſmall quantity of acid that is in it may well be ſuppoſed to be employed in forming the *fixed air*, which is always found in this proceſs: for that there is one common principle of acidity, and that all the acids are convertible into one another (at leaſt the nitrous acid into fixed air) is by no means an improbable ſuppoſition, though we are not yet in poſſeſſion of any proceſs by which it may be done. It is pretty evident that, in this reſpect, nature actually does what we are not able to do.

In my laſt Volume of Experiments, I recited the particulars of one, the reſult of which ſeemed to be diſſimilar to this with the ſcales of iron and inflammable air; for heating *red precipitate* in inflammable air, I then found little or no water; but having uſed more precautions, I have ſince found it in ſufficient quantity in this proceſs, even though the inflammable air was previouſly well dried with fixed ammoniac. In this experiment I diſcontinued the proceſs after three ounce meaſures of air were abſorbed, leaving room in the veſſel, that the moiſture might be more eaſily collected. With this precaution, and warming the veſſel, I collected between an half and three-fourths of a grain of water.

This experiment may be thought to be unfavourable to my preſent hypotheſis, as all water was carefully excluded, and yet a ſufficient quantity was found in the proceſs. But beſides taking into the account the water that is neceſſary to conſtitute the inflammable air, why may not *red precipitate*, in its dryeſt ſtate, be ſuppoſed to contain water, as well as the ſcales of iron, which will bear any degree of heat without parting with it. Red precipitate is made by a liquid proceſs, and therefore the water, that may enter into its compoſition as a calx, may quit it when it becomes a metal.

I ſhall take the liberty to obſerve farther, that the doctrine of the *decompoſition of water* being ſet aſide, that of *phlogiſton* (which, in conſequence of the late experiments on water, has been almoſt univerſally abandoned) will much better ſtand its ground, as all the newly diſcovered facts are more eaſily explained by the help of it.

If water be not decompoſed, both metals and ſulphur do certainly yield inflammable air, when ſteam is made to paſs over them in a red heat. They cannot, therefore, be *ſimple ſub-*

ſtances, as the antiphlogiſtic theory makes them to be. Alſo, the ſame thing that they have parted with, *viz.* inflammable air (or rather ſomething that is left of inflammable air when the water is taken from it, and which may as well be called phlogiſton as any thing elſe) may be transferred to other ſubſtances, and thus contribute to form any of the metals, ſulphur, phoſphorus, or any thing elſe that has been deemed to contain phlogiſton. This phlogiſton alſo, no doubt, having weight, it perfectly correſponds to the definition of a *ſubſtance*, having certain affinities, by means of which it is transferred from one body to another, as much as the different acids.

If there be no ſuch thing as one principle of phlogiſton, transferable from one ſubſtance to another, it muſt be admitted, that inflammable air from ſulphur is real ſulphur and water, that from iron, iron and water, as well as that very different ſubſtance, the *ſcale of iron.* And ſince copper, or any other metal, may be made of inflammable air from iron, &c. all the metals will be, in fact, convertible into one another. At leaſt, it may be ſaid, that all the component parts of any one metal may be ſo incorporated with any other, that no teſt can detect it. Alſo iron, made of inflammable air from ſulphur, ought, upon this hypotheſis, to have the properties of *ſulphurated iron*, which undoubtedly it would not have. An hypotheſis loaded with theſe difficulties muſt be inadmiſſible; whereas that of phlogiſton is extremely ſimple, and, as far as appears, of univerſal application.

The diſcovery that the greateſt part of the weight of inflammable air, as well as of other kinds of air, is water, does not make the uſe of the term phlogiſton leſs proper: for it may be ſtill given to that *principle*, or thing, which, when added to water, makes it to be inflammable air; as the term *oxygenous principle* may be given to that thing which, when it is incorporated with water, makes dephlogiſticated air.

As there is ſomething in dephlogiſticated air that ſeems to be the principle of *univerſal acidity*, ſo I am ſtill inclined to think, as I obſerved in my laſt Volume of Experiments, that phlogiſton is the principle of *alkalinity*, if ſuch a term may be uſed; eſpecially as alkaline air may be converted into inflammable air.

In the courſe of experiments recited in this Paper, I diſcovered more completely than before the ſource of my former miſtake, in ſuppoſing that fixed air was a neceſſary part of the produce of red lead, and alſo of manganeſe. Both theſe ſubſtances, I find, give of themſelves only dephlogiſticated air, and that of the pureſt kind; and all the fixed air they yielded in my former experiments muſt have come from the gun-barrel I then made uſe of, which would yield inflammable air, which, with dephlogiſticated air, forms fixed air. For though the dephlogiſticated air from red lead was ſo pure that, mixed with two meaſures of nitrous air, the three meaſures were reduced to five hundredth parts of a meaſure, and the ſubſtance gave no fixed air at all when it was heated in an earthen tube or retort; yet by mixing iron filings with it, or with manganeſe, as I had formerly done with red precipitate, I got more or leſs fixed air at pleaſure, and ſometimes no dephlogiſticated air at all.

XII. *Some Obſervations on the Irritability of Vegetables.*

By James Edward Smith, *M. D. F. R. S.*

Read February 14, 1788.

HAVING often heard that the ſtamina of the Barberry, *Berberis communis*, were endued with a conſiderable degree of irritability, I made the experiment in Chelſea Garden, May 25, 1786, on a buſh then in full flower. It was about one o'clock P. M. the day bright and warm, with little wind.

The ſtamina of ſuch of the flowers as were open were bent backwards to each petal, and ſheltered themſelves under their concave tips. No ſhaking of the branch appeared to have any effect upon them. With a very ſmall bit of ſtick I gently touched the inſide of one of the filaments, which inſtantly ſprung from the petal with conſiderable force, ſtriking its anthera againſt the ſtigma. I repeated the experiment a great number of times; in each flower touching one filament after another, till the tips of all ſix were brought together in the center over the ſtigma.

I took home with me three branches laden with flowers, and placed them in a jar of water, and in the evening tried the experiment on ſome of theſe flowers, then ſtanding in my room, with the ſame ſucceſs.

In order to diſcover in what particular part of the filaments this irritability reſided, I cut off one of the petals with a very fine pair of ſciſſars, ſo carefully as not to touch the ſtamen which ſtood next it: then, with an extremely ſlender piece of quill, I touched the outſide of the filament which had been next the petal, ſtroaking it from top to bottom; but it remained perfectly immoveable. With the ſame inſtrument I then touched the back of the anthera, then its top, its edges, and at laſt its inſide; ſtill without any effect. But the quill being carried from the anthera down the inſide of the filament, it no ſooner touched that part than the ſtamen ſprung forwards with great vigour to the ſtigma. This was often repeated with a blunt needle, a fine briſtle, a feather, and ſeveral other things, which could not poſſibly injure the ſtructure of the part, and always with the ſame effect.

To ſome of the antheræ I applied a pair of ſciſſars, ſo as to bend their reſpective filaments with ſufficient force to make them touch the ſtigma; but this did not produce the proper contraction of the filament. The incurvation remained only ſo long as the inſtrument was applied; on its being removed, the ſtamen returned to the petal by its natural elaſticity. But on the ſciſſars being applied to the irritable part, the anthera immediately flew to the ſtigma, and remained there. A very ſudden and ſmart ſhock given to any part of a ſtamen would, however, ſometimes have the ſame effect as touching the irritable part.

Hence it was evident, that the motion above deſcribed was owing to an high degree of irritability in the ſide of each filament next the germen, by which, when touched, it contracts, that ſide becomes ſhorter than the other, and conſequently the filament is bent towards the germen. I could not diſcover any thing particular in the ſtructure of that or any other part of the filament.

This irritability is perceptible in ſtamina of all ages, and not merely in thoſe which are juſt about diſcharging their pollen. In ſome flowers which were only ſo far expanded that they would barely admit a briſtle, and whoſe antheræ were not near burſting, the filaments appeared almoſt as irritable as in flowers fully opened; and in ſeveral old flowers, ſome of whoſe petals with the ſtamina adhering to them were falling off, the remaining filaments, and even thoſe which were already fallen to the ground, proved full as irritable as any I had examined.

From ſome flowers I carefully removed the germen, without touching the filaments, and then applied a briſtle to one of them, which immediately contracted, and the ſtigma being out of its way, it was bent quite over to the oppoſite ſide of the flower.

Obſerving the ſtamina in ſome flowers which had been irritated returning to their original ſituations in the hollows of the petals, I found the ſame thing happened to all of them ſooner or later. I then touched ſome filaments which had perfectly reſumed their former ſtations, and found them contract with as much facility as before. This was repeated three or four times on the ſame filament. I attempted to ſtimulate in the midſt of their progreſs ſome which were returning, but not always with ſucceſs; a few of them only were ſlightly affected by the touch.

The purpoſe which this curious contrivance of nature anſwers in the private œconomy of the plant, ſeems not hard to be diſcovered. When the ſtamina ſtand in their original poſition, their antheræ are effectually ſheltered from rain by the concavity of the petals. Thus probably they remain till ſome inſect coming to extract honey from the baſe of the flower, thruſts itſelf between their filaments, and almoſt unavoidably touches them in the moſt irritable part: thus the impregnation of the germen is performed; and as it is chiefly in fine ſunny weather that inſects are on the wing, the pollen is alſo in ſuch weather moſt fit for the purpoſe of impregnation. It would be worth while to place a branch of the Barberry flower in ſuch a ſituation, as that no inſect, or other irritating cauſe, could have acceſs to it; to watch whether in that caſe the antheræ would ever approach the ſtigma, and whether the ſeeds would be prolific.

I have been the more particular in theſe obſervations upon the Barberry, becauſe although ſeveral authors mention the irritability of its ſtamina, none, that I can find, have related in what part of the ſtamina this property reſides, or the purpoſe it ſerves; at leaſt they have not purſued their inquiries with any great degree of accuracy, but ſeem moſtly to have copied one another. GMELIN, who has written a diſſertation expreſſly on the irritability of vegetables, has ſcarcely any thing new on the ſubject; the chief part of his work is a catalogue of plants which he found *not* to be irritable.

The Barberry is not the only plant which exhibits this phænomenon. The ſtamina of *Cactus Tuna*, a kind of Indian Fig, are likewiſe very irritable. Theſe ſtamina are long and ſlender, ſtanding in great numbers round the inſide of the flower. If a quill or feather be drawn through them, they begin in the ſpace of two or three ſeconds to lie down gently on one ſide, and in a ſhort time they are all recumbent at the bottom of the flower. The motions in *Dionæa muſcipula*, *Mimoſa ſenſitiva* and *pudica*, are too well known to be mentioned here. A ſimilar phænomenon has been obſerved, where indeed an obvious botanical analogy would lead one to expect it, in the *Droſera*.

See Dr. WITHERING's Botanical Arrangement of Britiſh Plants. All theſe movements are, I think, certainly to be attributed to irritability. We muſt be careful not to confound them with other movements, which, however wonderful at firſt ſight, are to be explained merely on mechanical principles. The ſtamina of the *Parietaria*, for inſtance, are held in ſuch a conſtrained curved poſition by the leaves of the calyx, that as ſoon as the latter become fully expanded, or are by any means removed, the ſtamina, being very elaſtic, fly up, and throw their pollen about with great force. I have lately obſerved a ſimilar circumſtance in the flowers of *Medicago falcata*. In this plant the organs of generation are held in a ſtraight poſition by the carina of the flower, notwithſtanding the ſtrong tendency of the infant germen to aſſume its proper falcated form. At length, when the germen becomes ſtronger, and the carina more open, it obtains its liberty by a ſudden ſpring, in conſequence of which the pollen is plentifully ſcattered about the ſtigma. The germen may at pleaſure be ſet at liberty by nipping the flower ſo as gently to open the carina, and the ſame effect will be produced.

As the foregoing experiments ſhew vegetables to poſſeſs irritability in common with animals, ſo there are plants which ſeem to be endued with a kind of ſpontaneous motion. LINNÆUS having obſerved that the Rue moves one of its ſtamina every day to the piſtillum, I examined the *Ruta chalepenſis*, which differs very little from the common Rue, and found many of the ſtamina in the poſition which he deſcribes, holding their antheræ over the ſtigma; while thoſe which had not yet come to the ſtigma were lying back upon the petals, as well as thoſe which, having already performed their office, had returned to their original ſituation. Trying with a quill to ſtimulate the ſtamina, I found them all quite devoid of irritability. They are ſtout, ſtrong, conical bodies, and cannot, without breaking, be forced out of the poſition in which they happen to be. The ſame phænomenon has been obſerved in ſeveral other flowers; but it is no where more ſtriking or more eaſily examined than in the Rue.

I could wiſh to find an inſtance of this ſpontaneous motion combined with irritability in one and the ſame plant; but, I confeſs, I do not know one. From analogy I ſhould think it not impoſſible that the *Dionæa muſcipula*, and perhaps the *Droſeræ*, may have the ſame motion in their ſtamina as the *Ruta*, *Parnaſſia*, and *Saxifraga*, while their leaves poſſeſs irritability. But if this be the caſe, the ſeats of theſe two properties, being ſo different and remote from each other, ſhould ſeem to have as little connexion as if in two different plants. There ſtill remains then this difference between animals and vegetables, that although ſome of the latter poſſeſs irritability, and others ſpontaneous motion, even in a ſuperior degree to many of the former, yet thoſe properties have hitherto in animals only been found combined in one and the ſame part. Even *Sertulariæ* are not an exception to this obſervation. The greater part of their ſubſtance, indeed, reſembles that of plants in being indefinitely extended, and in wanting irritability and ſpontaneous motion. But their animated flowers or polypes, in which the eſſence of their being reſides, are endued with both theſe properties in an high degree.

I know it is the opinion of ſome philoſophers that a certain degree of irritability muſt pervade every part of vegetables, as the propulſion of their fluids cannot well be conceived to be accompliſhed by any other means. In a converſation on this ſubject with the celebrated M. Bonnet, of Geneva, he informed me, that he is ſtrongly of this opinion; and that he ſhould not deſpair, by throwing acid or other ſtimulating injections into the veſſels of ſome plants, of ſeeing with a microſcope at once the propulſion of the ſap, and the contractions by which it is performed. He urged me, with that amiable enthuſiaſm for which he is remarkable, to purſue the inquiry. Whether I do ſo or not, I think the idea too intereſting to be kept to myſelf, and ſhould be glad to ſee it realized by any one who has time and abilities for ſuch inveſtigations, who has accuracy and coolneſs in making his experiments, as well as fidelity and impartiality in recording them.

I cannot conclude this Paper without taking notice of another very curious property which vegetables ſeem to poſſeſs in common with animals, although certainly in a very inferior degree: I mean, that property, to uſe the words of Mr. HUNTER, who has ſtudied this principle to a vaſt extent in the animal œconomy, by which their conſtitution is capable only of a certain degree of action conſiſtently with health; when that degree is exceeded, diſeaſe or death is the conſequence. It is only by the help of this principle that I can explain why many plants reſiſt a great degree of cold for ſeveral winters before flowering; but, after that critical event, they periſh at the firſt approach of cold, and can by no art be preſerved ſo as to ſurvive the winter. But a more curious inſtance is that mentioned by LINNÆUS, without an explanation, in his Diſſertation on the Sexes of Plants, of the long duration of the piſtilla in the female hemp, while unexpoſed to the male pollen; whereas thoſe to which the pollen had acceſs immediately faded and withered away. In this caſe, I cannot help thinking, that in thoſe piſtilla on which the pollen had acted, and which conſequently had performed the function for which they were deſigned, the vital principle was much ſooner exhauſted than in thoſe which had known no ſuch ſtimulus. It is, perhaps, for the ſame reaſon that double flowers, in which, the organs of generation being obliterated, no impregnation can take place, laſt much longer in perfection than ſingle ones of the ſame ſpecies, as is notoriouſly the caſe with Poppies, Anemonies, &c. In ſingle Poppies the corolla falls off in a few hours; but in double ones it laſts ſeveral days; and this may poſſibly, combined with other obſervations, lead to a diſcovery of the real uſe of the corolla of plants, and the ſhare it has in the impregnation, about which there has yet been no probable conjecture.

XIII. *An Account of Experiments made by* Mr. JOHN M^c NAB, *at* Albany Fort, Hudſon's Bay, *relative to the Freezing of Nitrous and Vitriolic Acids. By* Henry Cavendiſh, *Eſq. F. R. S. and A. S.*

Read February 28, 1788.

FROM the experiments made by Mr. M^c NAB, of which I gave an account in the LXXVIth Volume of the Philoſophical Tranſactions, p. 241. it appeared, that ſpirit of nitre was ſubject, not only to what I call the aqueous congelation, namely, that in which it is chiefly, and perhaps intirely, the watery part which freezes, but alſo to another kind, in which the acid itſelf freezes, and which I call the ſpirituous congelation. When its ſtrength is ſuch as not to diſſolve ſo much as $\frac{243}{1000}$ of its weight of marble, or when its ſtrength is leſs than ,243, as I call it for ſhortneſs, it is liable to the aqueous congelation ſolely; and it is only in greater ſtrengths that the ſpirituous congelation can take place. This ſeems to be performed with the leaſt degree of cold when the ſtrength is ,411, in which caſe the freezing point is at – 1°½. When the acid is either ſtronger or weaker, it requires a greater degree of cold; and in both caſes the frozen part ſeems to approach nearer to the ſtrength of ,411 than the unfrozen part. The freezing points, anſwering to different degrees of ſtrength, ſeemed to be as follows.

Strength.

Strength.	Freezing point.	
,54	−31½°	ſpirituous congelation.
,411	− 1½	
,38	−45¼	
,243	−44¼	aqueous congelation.
,21	−17	

As ſome of theſe properties, however, were deduced from reaſoning not ſufficiently eaſy to ſtrike the generality of readers with much conviction, Mr. M^c^ NAB was deſired to try ſome more experiments to aſcertain the truth of it; which he was ſo good as to undertake, and has executed them with the ſame care and accuracy as the former.

For this purpoſe, I ſent him ſome bottles of ſpirit of nitre of different ſtrengths, and he was deſired to expoſe each of theſe liquors to the cold till they froze; then to try their temperature by a thermometer; afterwards to keep them in a warm room till the ice was almoſt melted, and then again expoſe them to the cold, and, when a conſiderable part of the acid had frozen, to try the temperature a ſecond time; then to decant the unfrozen part into another bottle, and ſend both parts back to England, that their ſtrength might be examined.

The intent of this ſecond expoſure to the cold was as follows. Spirit of nitre bears, like other liquors, to be cooled greatly below its freezing point without freezing: then the congelation begins ſuddenly; the liquor is filled with fine ſpicula of frozen matter, and the ice becomes ſo looſe and porous, that, if the proceſs be continued long enough for a conſiderable portion of the acid to congeal, ſcarce any of the fluid part can be decanted: whereas, if it be heated in this ſtate till the frozen part is almoſt, but not intirely, melted, and be again expoſed to the cold, as the liquor is then in contact with the congealed matter, it begins to freeze as ſoon as it arrives at the freezing point, and the ice becomes much more ſolid and compact.

The intent of decanting the fluid part, and ſending both parts back, that their ſtrength might be determined, was partly to examine the truth of the ſuppoſition laid down in my former Paper, that the ſtrength of the frozen part approaches nearer to ,411 than that of the unfrozen; but it is alſo a neceſſary ſtep towards determining the freezing point anſwering to a given ſtrength of the acid; for as the frozen part is commonly of a different ſtrength from the unfrozen, the ſtrength of the fluid part, and the cold neceſſary to make it freeze, is continually altering during the progreſs of the congelation. In conſequence of this, the temperature of the liquor is not that with which the frozen part congealed; but it is that neceſſary to make the remainder, or the fluid part, begin to freeze, or, in other words, it is the freezing point of the fluid part. This is the reaſon that a thermometer, placed in ſpirit of nitre, continually ſinks during the progreſs of congelation; which is contrary to what is obſerved in pure water, and other fluids in which no ſeparation of parts is produced by freezing.

Moreover, from the above-mentioned experiments of Mr. Mc Nab it appeared, that oil of vitriol, as well as ſpirit of nitre, is ſubject to the ſpirituous congelation; but it ſeemed uncertain, whether, like the latter, it had any point of eaſieſt freezing, or whether it did not uniformly freeze with leſs cold as the ſtrength increaſed. For this reaſon, ſome bottles of oil of vitriol, of different ſtrengths, were ſent, which he was deſired to try in the ſame manner as the former. This point, indeed, has ſince been determined by Mr. Keir, who has ſhewn that oil of vitriol has a ſtrength of eaſieſt freezing; and that at that point a remarkably ſlight degree of cold is ſufficient for its congelation.

The reſult of Mr. M^c^ NAB's experiments on the nitrous acid is given in the following table.

N°	Decanted part.		Undecanted part.		Strength of the whole maſs.	Strength before ſent.	Freezing point by firſt method.	Freezing point by ſecond method.
	Quantity.	Strength.	Quantity.	Strength.				
6	- -	- -	- -	- -	- -	,561	−41,6	- -
7	1410	,445	2137	,435	,439	,437	+*1,7	− 3,8
8	1658	,390	1940	,422	,407	,408	− 3,5	− 4
9	1368	,353	2438	,416	,393	,391	− 4,5	−11
10	2206	,343	1920	,373	,357	,357	−12,5	−13,8
11	3620	,310	602	,381	,320	,320	−22,5	−23
12	2155	,276	1494	,293	,283	,280	−39,1	−40,3
13	1618	,241	1961	,235	,238	,238	−34	−32

The firſt column contains the numbers by which Mr. M^c^ NAB has diſtinguiſhed the different bottles. The ſecond and third columns contain the quantity and ſtrength of the decanted part of the liquor; and the fourth and fifth ſhew the quantity and ſtrength of the undecanted part of the liquor. The ſixth column gives the ſtrength of both parts put together, or the ſtrength of the whole maſs; and the ſeventh is the ſtrength of the ſame acid, as it was determined before it was ſent to Hudſon's Bay. The ſtrengths of the decanted and undecanted parts were found by ſaturating the liquor returned home with marble; and that of the whole maſs was inferred by computation from the quantity and ſtrength of the decanted and undecanted parts; and as the ſtrength thus inferred never differs from that determined before the liquors were ſent to Hudſon's Bay by more than $\frac{1}{100}$ part of the whole, it is not likely that the ſtrengths of the decanted and undecanted parts here ſet down ſhould differ from the truth by much more than that quantity.

The eighth column contains the freezing points found in the firſt method, or the temperature of the liquors after the haſty congelation which took place on expoſing them to the cold without any frozen matter in them; and the ninth contains their temperature after the more gradual congelation which took place when they were cooled with ſome frozen matter in them; and as the unfrozen part of the acid was decanted immediately after the temperature had been obſerved, it follows, that this column ſhews the true freezing points of the decanted liquors. In like manner the eighth column ſhews the freezing points of that part of the liquor which remained fluid in the firſt manner of trying the experiment; but as the ſtrength of this part was not determined, the preciſe ſtrengths to which theſe freezing points correſpond are unknown. Thus much, however, is certain, that theſe points muſt be below thoſe of the whole maſs, and in all probability muſt be above thoſe of the decanted liquor; as there is great reaſon to think, that the quantity of frozen matter was always leſs, and conſequently the ſtrength of the fluid part differed leſs from that of the whole maſs, in the firſt way of trying the experiment than in the ſecond.

Before I draw any concluſions from theſe experiments, it will be proper to take notice of ſome particularities which occurred in trying them.

N° 6. was made to congeal by a freezing mixture of ſnow and diluted oil of vitriol. By the time the acid was cooled to −42°, icy filaments were formed on the inſide of the phial above the acid. Ten minutes after, the acid being cooled one degree more, the phial was taken out and agitated. This mixed the icy filaments with the acid, and made it freeze, which it ſeems not to have done before, in conſequence of which its temperature roſe to – 41°½. After having melted the greateſt part of theſe filaments, and again expoſed it to the freezing mixture, ſome ſnow accidentally fell into the acid, and made an uncertainty in the freezing point, for which reaſon it is not ſet down. But as it is evident, that the quantity of congealed matter in the firſt experiment was exceſſively ſmall, the ſtrength of the unfrozen part could not differ ſenſibly from that of the whole maſs, and therefore – 41°½ is the true freezing point that anſwers to the ſtrength of ,561.

It is remarkable, that N° 8. acquired by congelation a bluiſh colour, not unlike that which the dephlogiſticated nitrous acid, in Mr. M^{c} NAB's former experiments, acquired by dilution with ſnow. It is not ſaid, how long the acid retained this colour, but it was intirely gone when the phial arrived in England. I am quite at a loſs to account for this phænomenon, and why it happened to this bottle only.

N° 12. when cooled to – 17° ſeemed to contain many icy particles; but as it afterwards bore to be cooled to – 48°, without their increaſing, we may conclude, that they were not frozen ſpirit of nitre, but only ſome heterogeneous matter ſeparated from it. A little of the congealed part of N° 8. dropped into it while at this point, made it freeze, and it roſe to – 39°.

In all the foregoing acids the ice was heavier than the fluid part, and in conſequence ſubſided to the bottom; a proof that it was the ſpirituous congelation which had taken place in them: but in N° 13. the frozen part ſwam at top, which ſhews, that the congelation was of the aqueous kind.

It may appear remarkable to thoſe who read Mr. M^c^ NAB's experiments, that theſe acids bore to be heated ſo much above their freezing points before the ice intirely diſſolved. N° 6. bore to be heated 18 degrees, N° 7. 13 degrees, and N° 12. 17 degrees above their freezing points, before all the congealed acid had diſappeared. But as, in order to diſſolve this congealed matter, they were brought into a room in all probability a great many degrees warmer than the points to which they were heated, ſo that the liquors heated faſt; and as during the diſſolution the ice would ſubſide to the bottom; it is not extraordinary, that the fluid part in the phial might be many degrees warmer than the frozen part, unleſs the phials were much agitated during the time, which nothing ſhews them to have been; eſpecially if we conſider the great quantity of heat which, in all probability, muſt be communicated to the frozen acid in order to melt it; and that, perhaps, the frozen acid may receive and part with its heat but ſlowly. It muſt be obſerved, that in N° 6. and 12. the frozen part might very likely be of a conſiderably different ſtrength, and in conſequence its freezing point might be ſeveral degrees different from that of the whole maſs, ſo that the temperature to which the fluid was heated, in order to melt the ice, might very likely not differ ſo much from the freezing point of the ice itſelf as is here ſet down. But this could not be the caſe with N° 7.

It muſt be obſerved, that when Mr. M^c^ NAB wanted to try the temperature of N° 7. after it had frozen in the firſt manner, the ſtopper ſtuck ſo tight that he was not able to remove it without warming it before the fire. The thermometer was then introduced, and ſtood ſeveral minutes therein at + 1°½, or + 2°. As the thermometer remained ſo long at this point, one

might

might naturally ſuppoſe, that this was the true freezing point of the unfrozen acid. But yet, from what has been juſt ſaid, it ſeems not improbable that it may be otherwiſe, and that the true freezing point may be ſenſibly lower; for which reaſon it is marked in the table with an aſteriſk (*) as doubtful.

It was before ſaid, that the temperatures in the ninth column of the foregoing table, are the freezing points anſwering to the ſtrengths expreſſed in the third column, and that − 41°½ is the freezing point anſwering to the ſtrength of ,561; whence the freezing points determined by theſe experiments, and their reſpective ſtrengths, are as follows:

Strength.	Freezing point.
,561	$-4\dot{1},6$
,445	− 3,8
,390	− 4
,353	−11
,343	−13,8
,310	−23
,276	−40,3

By interpolation from theſe *data*, according to Newton's method †, it appears, that the ſtrength at which the acid freezes with the leaſt cold is ,418, and that the freezing point anſwering to that ſtrength is − 2°$\frac{4}{10}$.

In order to ſhew more readily the freezing point anſwering to any given ſtrength, I have computed, by the ſame method, the following table, in which the ſtrengths increaſe in arithmetical progreſſion.

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† Princip. Math. Lib. III. prop. 40. lem. 5.

Strength.	Freezing point.	Difference.
,568	−45,5	+15,4
,538	−30,1	+12
,508	−18,1	+ 8,7
,478	− 9,4	+ 5,3
,448	− 4,1	+ 1,7
,418	− 2,4	− 1,8
,388	− 4,2	− 5,5
,358	− 9,7	− 8
,328	−17,7	− 10
,298	−27,7	

It was before ſhewn, that the freezing points, found by the firſt method, ought to be below thoſe of the whole maſs, and muſt, in all probability, be above thoſe of the decanted liquor. In order to ſee how this agrees with obſervation, I computed in the above-mentioned manner the freezing points anſwering to the ſtrength of the whole maſs, and compared them with the obſerved freezing points. The reſult is given in the following table.

N°	Strength of the whole maſs.	Strength of the decanted liquor.	Computed freezing point of the whole maſs.	Obſerved freezing point.	
				In firſt method.	In ſecond method.
7	,439	,445	− 3,2	+ 1,7	− 3,8
8	,407	,390	− 2,6	− 3,5	− 4,
9	,393	,353	− 3,7	− 4,5	−11,
10	,357	,343	−10,	−12,5	−13,8
11	,320	,310	−19,9	−22,5	−23,
12	,283	,276	−35,6	−39,1	−40,3

It may be obſerved, that the freezing point of N° 7. tried in the firſt way, is conſiderably above that correſponding to the ſtrength of the whole maſs; but as this experiment was ſhewn (in p. 173.) to be doubtful, and not unlikely to exceed the truth, we may ſafely reject it as erroneous. All the others, as might be expected, are lower than thoſe correſponding to the ſtrength of the whole maſs, and above thoſe obſerved in the ſecond manner, and therefore ſerve to confirm the truth of the above determination of the freezing points of ſpirit of nitre; and alſo ſhew, that in this acid the point of ſpirituous congelation is pretty regular, and does not depend much, if at all, on the rapidity with which the congelation is performed.

The point of aqueous congelation, however, ſeems liable to conſiderable irregularity; for N° 13 after having been expoſed to the cold, froze on agitation, the congelation, as was before ſaid, being of the aqueous kind, and the thermometer ſtood ſtationary therein at −34°. The ice being then almoſt melted, it was again expoſed to the cold, till a good deal was frozen; but yet its temperature was then no lower than −32°¼, though the quantity of frozen matter muſt certainly have been much more than in the firſt trial. The fluid part being then decanted, and the frozen part melted, both were again expoſed to the cold. They both were made to congeal by agitation, and the temperature of the undecanted was then found to be −35°, and that of the decanted part −37°: ſo that it ſhould ſeem as if the freezing point found by the haſty congelation was always lower than that found the other way, which may, perhaps, proceed from this cauſe; namely, that when ſufficient time is allowed, the watery part will ſeparate from the reſt, and freeze in a degree of cold much leſs than what is required to produce that effect, when it is performed in a more rapid manner.

Theſe experiments confirm the truth of the concluſions I drew from Mr. M^c NAB's former experiments; for, firſt, there is a certain degree of ſtrength at which ſpirit of nitre freezes with a leſs degree of cold than when it is either ſtronger or weaker; and when ſpirit of nitre, of a different ſtrength from that, is made to congeal, the frozen part approaches nearer to the foregoing degree of ſtrength than the unfrozen. Likewiſe this ſtrength, as well as the freezing point correſponding thereto, and the freezing point anſwering to the ſtrength of ,54, come out very nearly the ſame as I concluded from thoſe experiments; for by the preſent experiments they come out ,418, $-2°\frac{4}{10}$, and $-31°$, and by the former ,411, $-1°\frac{1}{2}$, and $-31°$. But the freezing point anſwering to the ſtrength of ,38 is totally different from what I there ſuppoſed. This muſt have been owing to the ſtrength of that acid having been very different from what I thought it; which is not improbable, as its ſtrength was inferred only from the quantity of ſnow which was added to it in finding the degree of cold produced by its mixture with ſnow.

After the foregoing experiments were finiſhed, Mr. M^c NAB made ſome more for determining the freezing points both of the decanted and undecanted part; but for want of a ſufficient explanation of the manner in which they were executed, I have not been able to make any uſe of them. In their preſent ſtate they ſhew much appearance of irregularity; but this would very likely have been cleared up, if the circumſtances had been more fully detailed.

On

On the Vitriolic Acid.

An irregularity of a remarkable kind occurred in trying two of theſe acids; namely, when the undecanted part was melted and again made to congeal, its freezing point was found to be much leſs cold than that of the decanted part, and the difference was much greater than could be attributed to the difference of ſtrength. This ſeems to have happened only in the two ſtrongeſt acids, namely, N° 1. and 2. and in great meaſure confirms the ſuppoſition which I formed from Mr. M^c^ NAB's former experiments, that the congealed part of oil of vitriol differs from the reſt, not merely in ſtrength, but alſo in ſome other reſpect, which I am not acquainted with. It ſhould ſeem, however, that this property does not extend to weak oil of vitriol.

It perhaps may be ſuſpected, that this property takes place in the nitrous acid alſo, and was the cauſe of the ſlow melting of the ice taken notice of in p. 172. But I think it more likely, that that phænomenon proceeded from the cauſes there aſſigned.

Some ſmaller irregularities occurred in trying the vitriolic acid, the cauſe of which I believe was, that when this acid has been cooled below the freezing point, and begins to freeze, the congelation proceeds but ſlowly; ſo that a conſiderable time elapſes before it riſes to the true freezing point. Something of the ſame kind ſeems to take place in the nitrous acid alſo, though in a leſs degree; for the decanted liquors uſually continued to freeze and depoſit a ſmall quantity of ice, for a few minutes after they were poured off, though their cold, at leaſt in ſome inſtances, was found rather to diminiſh during that time.

It muſt be obſerved, that ſmall ſpicula of ice always came over along with the decanted liquor; and to this, in all probability, the new-formed ice attached itſelf; for otherwiſe it is likely, that no ice would have been produced.

The following table contains the ſtrength of the acids as determined before they were ſent to Hudſon's Bay, and the quantity and ſtrength of the decanted and undecanted parts when they arrived at London, and the ſtrength of the whole maſs as computed from thence. For the ſake of uniformity, I have expreſſed their ſtrengths, like thoſe of the nitrous acid, by the quantity of marble neceſſary to ſaturate them, though I did not find their ſtrength by actually trying how much marble they would diſſolve; as that method is too uncertain, on account of the ſelenite formed in the operation, and which in good meaſure defends the marble from the action of the acid. The method I uſed was, to find the weight of the plumbum vitriolatum formed by the addition of ſugar of lead, and from thence to compute the ſtrength, on the ſuppoſition that a quantity of oil of vitriol, ſufficient to produce 100 parts of plumbum vitriolatum, will diſſolve 33 of marble; as I found by experiment that ſo much oil of vitriol would ſaturate as much fixed alkali as a quantity of nitrous acid ſufficient to diſſolve 33 of marble. It may be obſerved, that the quantity of alkali, neceſſary to ſaturate a given quantity of acid, can hardly be determined with much accuracy, for which reaſon the foregoing leſs direct method was adopted; eſpecially as the precipitation of plumbum vitriolatum ſhews the proportional ſtrengths, which is the thing principally wanted, with as great accuracy as any method I know.

N°	Strength before ſent.	Decanted part.		Undecanted part.		Strength of whole maſs.
		Quantity.	Strength.	Quantity.	Strength.	
1	,977	1375	,967	3460	,963	,964
2	,918	3915	,919	1876	,905	,914
3	,846	88	,777	4915	,850	,849
4	,758	389	,710	3795	,753	,755
				547	,803	

The undecanted part of N° 4. was divided into two parts; namely, the leſs and the more congealable part; and it is the latter whoſe quantity and ſtrength is given in the laſt line.

It is well known, that oil of vitriol attracts moiſture with great avidity; and ſome of theſe acids were much expoſed to the air during the experiments made with them, and may therefore be ſuppoſed to have attracted ſo much moiſture from the air, as might ſenſibly diminiſh their ſtrength; and this ſeems actually to have been the caſe with ſome of them. But as the bottles were well ſtopped, and as, except in one acid which was the moſt expoſed to the air, the ſtrength of the whole maſs comes out not much leſs than that determined before the liquors were ſent to Hudſon's Bay, I imagine their ſtrength could not ſenſibly alter during their voyage home; and conſequently their ſtrength, at the time the laſt obſervations were made with them, could not differ much from that here ſet down.

It would be tedious to give the experiments for determining their freezing points in detail; but the reſult is as follows. The freezing point of N° 1. tried in the firſt method, was ſomewhat above + 1°, but it is uncertain how much; that tried in the ſecond manner ſeemed − 6°½. But the freezing point of the undecanted part, after having been intirely melted, and again

expoſed

expoſed to the cold, was +9°. It muſt be obſerved, that though this part was in all probability at firſt ſtronger than the decanted part, yet at the time its freezing point was tried, it ſeems to have become rather weaker than that, owing to its expoſure to the air. It was before ſaid, that the freezing point tried in the ſecond manner is that of the decanted liquor; ſo that the freezing point of the decanted part ſeems to have been 13 or 14 degrees colder than that of the undecanted part; though the difference of ſtrength, if there was any, muſt in all probability have tended to produce the contrary effect.

The freezing point of N° 2. tried in the firſt way, was −26°; and that tried in the ſecond was −30°, or 26°; but yet the freezing point of the undecanted part was 26 or 30 degrees higher, namely, at zero; a difference which could ſcarcely have proceeded from the difference of ſtrength.

The freezing point of N° 3. could hardly differ much from +42°; and that of N° 4. was about −45°.

It ſhould be remarked, that when this laſt acid, as well as N° 1. and 2. were expoſed to a great cold, a ſediment formed in them. This muſt have been of a very different nature from frozen acid, as appeared both from its texture, which was ſoft and mucilaginous to the feel, inſtead of being gritty as the frozen acid always was; and alſo from its being not much increaſed by an increaſe of cold; and therefore ſeems to have been ſome impurity ſeparated from the acid. The quantity was greateſt in N° 4.; but even in this, though it appeared great, it is likely that the real quantity was very ſmall.

Another bottle of acid, whoſe ſtrength was ,659, was ſent; but Mr. M^c Nab was not able to make this freeze.

From theſe experiments it ſhould ſeem, that the freezing point of oil of vitriol, anſwering to different ſtrengths, is nearly as follows:

Strength.	Freezing point.
,977	+ 1°
,918	− 26
,846	+ 42
,758	− 45

From hence we may conclude, that oil of vitriol has not only a ſtrength of eaſieſt freezing, as Mr. KEIR has ſhewn; but that, at a ſtrength ſuperior to this, it has another point of contrary flexure, beyond which, if the ſtrength be increaſed, the cold neceſſary to freeze it again begins to diminiſh.

The ſtrength anſwering to this latter point of contrary flexure muſt, in all probability, be rather more than ,918, as the decanted or unfrozen part of N° 2. ſeemed rather ſtronger than the undecanted part; and for a like reaſon the ſtrength of eaſieſt freezing is rather more than ,846.

Mr. KEIR found that oil of vitriol froze, with the leaſt degree of cold, when its ſpecific gravity at 60° of heat was 1,780, and that the freezing point anſwering to that degree of ſtrength was +46°; which agrees pretty nearly with theſe experiments, as the ſtrength of oil of vitriol of that ſpecific gravity is ,848, that is, nearly the ſame as that of N° 3.

APPENDIX.

Tranſlation of Don Michael Rubin de Celis's *Letter to the Royal Society, relative to a Maſs of native Iron, found in* South-America. See p. 37.

ABOUT thirty years ago, the various barbarous nations who inhabited the provinces of the great Chaco Gualamba, expelled the Spaniards from thence; and ſince that time, the countries on the ſouthern part of the river Vermejo, and weſtern of the great river Paranà have been almoſt totally deſerted. The only employment of the few Indians who dwell within the juriſdiction of Santiago del Eſtero is to gather the honey and wax, with which the woods abound. Theſe Indians diſcovered, in the midſt of a wide-extended plain, a large maſs of metal, which they called pure iron; part of which projected above the ground about a foot, and almoſt the whole of its upper ſurface was viſible. Intelligence of this diſcovery was immediately communicated to the Viceroys of Peru. That ſuch a maſs of iron ſhould be found in a country where there are no mountains, nor even the ſmalleſt ſtone within a circumference of one hundred leagues, could not fail to appear extraordinary, notwithſtanding we know there are mines of pure iron in Europe. Some private perſons, at the great riſk of their lives, both from the uncertainty of procuring food or even water (of which none is to be found but what rain happens to be preſerved in ſome natural cavities of the earth), from the danger of meeting the roving Indians, from the various wild beaſts found in thoſe plains, ſuch as tygers, leopards, tapirs, from the ſwarms of poiſonous reptiles, and finally from the endleſs thickets, led on by hopes of enriching themſelves, boldly undertook the journey, to obtain ſome of the metal. They tranſmitted a part of it to Lima and Madrid, by which no other advantage was gained than to aſcertain it to be very ſoft and very pure iron. As it is forbidden by law, for political reaſons, to manufacture iron in that country, though different parts of it abound with iron mines; and as it was aſſerted, that the vein of iron extended many leagues, the viſible part being only its creſt projecting above the ground, which, when dug round, was found to meaſure three yards from N. to S. two yards and a half from E. to W. and about one-third of a yard in thickneſs; the Viceroy of the river Plata ſent me with orders to examine this diſcovery with accuracy; and, in caſe I found it a beneficial mine, that I ſhould eſtabliſh a colony there. I accordingly ſet off, well eſcorted, in the beginning of February, 1783, from Rio Salado, an ancient civilized hamlet of the Indians whom we call Vilelas, and purſued my journey in the direction E. ¼ N.E. though, upon further examination, I found that I ought to have taken my direction E. ¼ S.E. both corrected.

The aſpect of the country between the river and the mine, diſtant ſeventy leagues from the ſettlement, is curious: it conſiſts of an immenſe plain, alternately intermixed with thick woods and fertile fields, forming moſt pleaſing landſcapes.

The latitude of the mine I found, by obſervation, to be 27° 28′ S.

There is not one fixed place of habitation throughout the whole country, owing to a ſcarcity of running water. That which is drunk by the honey-gatherers, who reſide there in ſmall bodies the greateſt part of the year, collecting honey in the woods, is rain water, as I have already obſerved. Theſe, and a few roving tribes of barbarous Indians, who reſemble the Tartars in their way of life, and come hither, at a certain ſeaſon of the year, from the borders of the river Vermejo, in queſt of a wild root, which they call Koruu, and which they conſtantly chew, as a remedy againſt the peſtilential air of their native country, and alſo as a preſervative againſt the bite of poiſonous reptiles, are the only people ever ſeen in thoſe pleaſant and extenſive plains.

I arrived the 15th of February at the place called Otumpa, where the maſs was found almoſt buried in pure clay and aſhes.

The exterior appearance of it was that of perfectly compact iron; but upon cutting off pieces of it, I found the internal part full of cavities, as if the whole had been formerly in a liquid ſtate. I was confirmed in this idea, by obſerving on the ſurface of it the impreſſions as of human feet and hands of a large ſize, as well as of the feet of large birds, which are common in this country. Though theſe impreſſions ſeem very perfect, yet I am perſuaded that they are either a *luſus naturæ*, or that impreſſions of this nature were previouſly upon the ground, and that the liquid maſs of iron falling upon it received them. It reſembled nothing ſo much as a maſs of dough, which, having been ſtamped with impreſſions of hands and feet, and marked with a finger, was afterwards converted into iron.

I began to cut off part of it with chiſſels; and in ſeparating from the maſs twenty-five or thirty pounds, I ſpoiled all the chiſſels I had, to the number of ſeventy.

I ordered my men to dig round it, and found the under ſurface covered with a coat of ſcoriæ from four to ſix inches thick; undoubtedly occaſioned by the moiſture of the earth, becauſe the upper ſurface was clean.

Having moved it half round, by means of handſpikes, I ordered the ground under its bed to be dug to a conſiderable depth, and even blew it up in two places with gunpowder; after which, examining the deepeſt part of the earth, I found it exactly like the upper part, and of the ſame nature as the earth of all the country, as likewiſe of two pits, which I had dug at the diſtance of ſeventy or one hundred paces E. and W. of the maſs. Finding here no root or trace of generation, I reaſoned in the following manner.

Either this maſs was produced in the ſpot where it lies, or it was conveyed hither by human art, or caſt hither by ſome operation of nature. It could not be generated here, according to any known proceſs of nature. And whence, by whom, or how, could it be conveyed hither, as there are no iron mines within hundreds of leagues, nor remembrance that any have been worked in the kingdom? It could be of no value, ſince it could not be uſed; and why bring it into a country the moſt uninhabitable of all the Chaco, from the want of water? Beſides, how could ſo heavy a maſs be conveyed, the Indians never having known the uſe of wheel carriages? This maſs, therefore, muſt have been the effect of ſome volcanic exploſion.

Many circumſtances induce me to think ſo. Volcanos frequently leave behind them, after exploſion, pits of water, either hot or cold; and at the diſtance of about two leagues to the eaſt of this maſs, I diſcovered a brackiſh mineral ſpring, the only one to be found in all this country. In the whole diſtrict of the Chaco I travelled over, I obſerved no difference in the level of the ground, except the very ſpot where I made this diſcovery; and here only I found a gentle aſcent running from N. to S. and which attracted my notice before I had ſeen the maſs of iron. This aſcent is between four and ſix feet above the reſt of the country. The earth in every part around this maſs, as well as about the brackiſh ſpring, is a very light, looſe earth, like aſhes, even in colour. The graſs produced immediately contiguous, called Ahivi, is ſhort, ſmall, and extremely unpalatable to cattle; whereas the graſs found in the reſt of the country, at a little diſtance from the maſs and ſalt ſpring, is long and very grateful to them.

At a little depth in the earth are found ſtones of quartz, of a beautiful red colour, which the honey-gatherers make uſe of as flints to light their fires. They had formerly carried ſome of them away, on account of their peculiar beauty, being ſpotted and ſtudded as it were with gold. One of theſe, that weighed about an ounce, came into the hands of the Governor of Santiago del Eſtero, who told me, that he ground it, and ſhewed me more than a drachm of gold that he had extracted from it.

It is an undoubted fact, that in theſe immenſe foreſts there exiſts a maſs of pure iron, in the ſhape of a tree, with its branches. Many of the Indians have ſeen it; and the inhabitants of the colony of the Avipones, are acquainted with the ſpot where it lies. A diſtinguiſhed European of the city of Salta touched it. The body of this tree extends upon the ground in the direction from E. to W. having left behind to the E. a part which lies in the direction from N. to S. and it is from this that the pieces of metal may have been taken with a chiſſel. From this account one may venture to form the following hypotheſis.

I will ſuppoſe, that the volcanic exploſion happened in the ſpot where I diſcovered the brackiſh ſpring; that by the exploſion a great quantity of earth was raiſed, and formed the elevation of this part of the plain above the reſt; that it was originally much higher, but the continual rains of the Chaco, which is overflowed a third part of the year, are always acting to bring it to a level with the reſt of the country.

The direction of the greateſt portion of the maſs of iron ejected was from E. to W. and being heavier reached to the diſtance where it is found. Another portion of the matter ſmaller, and perhaps more fluid, ſeparated and took another direction, running into ſeveral ſtreams, as when water is thrown out of a pail. This ſmall portion of matter having cooled, and the earth which ſupported it being waſhed away gradually by the water, muſt, I apprehend, have formed what is now called the Tree of iron.

The ſaline and antimonial matters, which uſually accompany all minerals, muſt have been ſcattered round about in a ſimilar manner, and rendered the ground barren.

In the kingdom of Santa Fe de Bogotà, there is found duſt of platina mixed with gold. Almoſt every body knows the great affinity there is between theſe two metals, ſo that it is not ſurprizing, conſidering all theſe reaſons, that the fire of the volcano ſhould have melted the platina which lay above the gold, and thrown it up. This principle of volcanos is the

moſt

moſt natural to account for the formation of thoſe famous maſſes of ſilver found ſeparate at Guantajaia, about which ſo many extravagant and ridiculous ſtories have been told.

The maſs of iron, which is the ſubject of this letter, according to its cubic meaſure, and allowing it a little more ſpecific gravity than iron, muſt weigh about 300 quintals.

(Sig.) Chev. MICH. RUBIN DE CELIS.

From the Iſland of Leon,
June 20, 1786.

A

METEOROLOGICAL JOURNAL

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THE ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January 1787.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Jan. 1	8	0	37	45½	30,42	0,042	SW	1	Fine.
	2	0	43½	47½	30,39		SSW	1	Cloudy.
2	8	0	34	45	30,42		WSW	1	Foggy.
	2	0	36	45	30,40		WSW	1	Cloudy.
3	8	0	37	47½	30,34	0,037	SW	1	Cloudy.
	2	0	40	50	30,37		WSW	1	Cloudy.
4	8	0	40	48	30,51		WSW	1	Cloudy.
	2	0	44	51	30,53		WSW	1	Cloudy.
5	8	0	42½	50	30,45		WSW	1	Cloudy.
	2	0	47	52	30,42		WNW	1	Cloudy.
6	8	0	44	51	30,35		NW	1	Cloudy.
	2	0	47	52	30,37		NNE	1	Cloudy.
7	8	0	33	50½	30,60		NE	1	Fine.
	2	0	36	51	30,62		ENE	1	Fine.
8	8	0	27	48	30,64		ESE	1	Foggy.
	2	0	34	50	30,63		NE by N	1	Cloudy.
9	8	0	36	49	30,63	0,017	NNE	1	Foggy.
	2	0	40	50½	30,60		NNE	1	Cloudy.
10	8	0	35	49	30,44		SE	1	Cloudy.
	2	0	34½	50½	30,41		SE	1	Cloudy.
11	8	0	34½	48½	30,19		SE	1	Cloudy.
	2	0	40	50½	30,10		SE by S	1	Fine.
12	8	0	34½	49	29,83		SE	1	Fine.
	2	0	40	52	29,68		SE by E	2	Fine.
13	8	0	37	48	29,52		SE by S	2	Fine.
	2	0	45	50	29,59		SSE	2	Cloudy.
14	8	0	37	48	29,89		SE by E	1	Foggy.
	2	0	45	50	29,91		SE by E	1	Fine.
15	8	0	33½	48	30,04		SE	1	Foggy.
	2	0	34	50½	30,02		ESE	1	Foggy.
16	8	0	30	48	29,90		SE by S	1	Foggy.
	2	0	36	49	29,85		SE	1	Cloudy.

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for January 1787.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Wind.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Jan. 17	8	0	35½	47	30,07		NNE	1	Cloudy.
	2	0	40	49	30,16		NNE	1	Cloudy.
18	8	0	30	47	30,26		SW	1	Foggy.
	2	0	38	50	30,13		WSW	1	Rainy.
19	8	0	33	48	30,27	0,010	NW	1	Foggy.
	2	0	39½	50	30,25		NW	1	Foggy.
20	8	0	43	48	30,16	0,095	NW	1	Foggy.
	2	0	49	51	30,16		NNW	1	Cloudy.
21	8	0	44	49	30,16	0.018	NW	1	Foggy.
	2	0	47	52	30,12		NW by N	1	Cloudy.
22	8	0	43	49	30,12		NW	1	Cloudy.
	2	0	46	53	30,13		NNE	1	Cloudy.
23	8	0	41½	52	30,09	0,016	SW by S	1	Cloudy.
	2	0	43	53	30,08		W	1	Cloudy.
24	8	0	41	52	30,09		NNE	1	Foggy.
	2	0	44	54	30,10		E by N	1	Fine.
25	8	0	37	51½	30,15	0,029	ESE	1	Cloudy.
	2	0	38	53	30,18		SE by S	1	Fine.
26	8	0	28½	51	30,15		NE by E	1	Fine.
	2	0	33½	52	30,05		NE	2	Cloudy.
27	8	0	28	49½	29,84		NE	1	Snow.
	2	0	31	50	29,83		NE by E	1	Cloudy.
28	8	0	30	47½	29,67		SE by S	3	Cloudy.
	2	0	31½	49	29,64		SSE	2	Snow.
29	8	0	40½	47½	29,88	0,068	S by E	2	Cloudy.
	2	0	45	49	29,93		SSW	2	Cloudy.
30	8	0	47	49	30,27	0,028	SSW	2	Cloudy.
	2	0	51	51½	30,32		SSW	2	Fine.
31	8	0	38	50	30,38		SW	1	Fine.
	2	0	48½	53	30,38		SW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Feb. 1	8	0	41	50	30,31		SW	1	Fine.
	2	0	50	54	30,25		SW	1	Fine.
2	8	0	44	52	30,28	0,034	SE by E	1	Foggy.
	2	0	48	54	30,28		SSW	1	Cloudy.
3	8	0	45	53	30,11		SW	1	Cloudy.
	2	0	49	54½	30,01		SW	2	Cloudy.
4	8	0	35	53	30,25	0,046	SE by E	1	Fine.
	2	0	44	54	30,21		SE	1	Fine.
5	8	0	33	52½	30,04		SE by E	1	Cloudy.
	2	0	41	53	29,95		SE by E	1	Fine.
6	8	0	37	51	29,87		SW by S	1	Cloudy.
	2	0	44	53	29,82		SW	1	Cloudy.
7	8	0	48	52	29,55	0,020	S by W	2	Rainy.
	2	0	52	54	29,55		SSW	2	Cloudy.
8	8	0	37	52	30,01	0,018	SW	1	Fine.
	2	0	49	54	30,01		SW by W	2	Cloudy.
9	8	0	46½	53	29,77		SW by S	2	Rainy.
	2	0	50	54	29,61		SW by W	2	Cloudy.
10	8	0	43½	53	29,41	0,058	S by E	2	Cloudy.
	2	0	47	54	29,24		SSE	2	Rainy.
11	8	0	41	53	29,21	0,160	S by W	2	Fair.
	2	0	45	54	29,05		S by W	2	Rainy.
12	8	0	39½	53	28,68	0,206	SW by S	3	Rainy.
	2	0	45	53	28,67		SSW	3	Rainy.
13	8	0	41	52	28,71	0,376	SW	1	Cloudy.
	2	0	45½	53	28,99		W	1	Cloudy.
14	8	0	35	51	29,71		W by N	1	Fine.
	2	0	45	54	29,86		W by N	1	Fine.
15	8	0	46	52	29,83		SW by W	2	Cloudy.
	2	0	49	51	29,81		SW	2	Cloudy.
16	8	0	49	53	29,96	0,026	SW	2	Cloudy.
	2	0	51	55	29,85		SW	2	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain .	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Feb. 17	8	0	38	54	30,31		SW	1	Fine.
	2	0	47	56	30,36		W	1	Fine.
18	8	0	42	54	30,30		SW by S	1	Cloudy.
	2	0	50	55	30,27		W	1	Cloudy.
19	8	0	39	54	30,34		SW	1	Fine.
	2	0	52	56	30,33		SW by W	1	Fine.
20	8	0	40	54	30,27		S by E	1	Foggy.
	2	0	45	55	30,27		SE by S	1	Foggy.
21	8	0	41½	54	30,18		N	1	Fine.
	2	0	46	55	30,16		NNE	1	Cloudy.
22	8	0	33½	53	30,16		ENE	1	Fine.
	2	0	41	55	30,16		E	1	Fine.
23	8	0	31	52	30,14		ESE	1	Foggy.
	2	0	41	55	30,14		ESE	1	Fine.
24	8	0	36	52	30,08		W	1	Cloudy.
	2	0	44	53	30,06		W	1	Cloudy.
25	8	0	38	52	30,08		W	1	Cloudy.
	2	0	46	53	30,03		W	1	Cloudy.
26	8	0	42	52	30,06	0,024	W by N	1	Cloudy.
	2	0	51	53	30,08		W by S	1	Cloudy.
27	8	0	40	51	30,11		SW by S	1	Fine.
	2	0	49	53	30,00		SW	1	Fine.
28	8	0	43	51	29,90	0,073	W	1	Fine.
	2	0	53	54	29,97		W by S	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
March 1	7	0	51	53	29,80	0,107	SW	2	Cloudy.
	2	0	55	55	29,62		SW	2	Rainy.
2	7	0	49	54	29,32	0,036	SW by S	3	Cloudy.
	2	0	53	54	29,38		SW by S	2	Cloudy.
3	7	0	46	53½	29,45		SW by S	2	Cloudy.
	2	0	49	54	29,03		SSW	2	Rainy.
4	7	0	42	53	29,10	0,076	W	1	Fair.
	2	0	45	54	29,55		NNW	1	Cloudy.
5	7	0	36	52	29,75		SE	1	Cloudy.
	2	0	37	53	29,55		SE by E	1	Rainy.
6	7	0	44	52	29,44	0,061	SSE	2	Cloudy.
	2	0	47	54	29,11		S by E	2	Rainy.
7	7	0	40	53	29,31	0,261	WNW	1	Fair.
	2	0	49	55	29,52		WNW	1	Fine.
8	7	0	34	53	29,88		WNW	1	Fine.
	2	0	47½	54½	29,92		NW	1	Fine.
9	7	0	38	53	29,53		SE	2	Hazy.
	2	0	45	54	29,45		SSE	2	Cloudy.
10	7	0	40	53	29,17		S	2	Rainy.
	2	0	49	55	28,93		SW	2	Rainy.
11	7	0	40	52½	29,85	0,278	W	1	Fine.
	2	0	46½	54½	30,01		W by S	1	Cloudy.
12	7	0	35	51½	30,37	0,022	W	1	Foggy.
	2	0	47	54½	30,41		W	1	Fine.
13	7	0	41½	52	30,51		W by S	1	Cloudy.
	2	0	52	54½	30,51		WSW	1	Cloudy.
14	7	0	45	53½	30,51		WSW	1	Cloudy.
	2	0	49	55	30,51		NW	1	Cloudy.
15	7	0	41½	53	30,42		W	1	Fine.
	2	0	55	55	30,40		W	1	Cloudy.
16	7	0	41	54	30,38		W	1	Fine.
	2	0	52	55	30,33		W	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
March 17	7	0	46	53	30,28		WNW	1	Fine.
	2	0	51	55	30,29		NWbyW	1	Cloudy.
18	7	0	43	53	30,50		NNW	1	Fine.
	2	0	52	50	30,49		NNW	1	Fine.
19	7	0	38	52	30,41		W	1	Fine.
	2	0	53	55	30,31		W by S	1	Fine.
20	7	0	40	53	30,24		SW	1	Foggy.
	2	0	54	56	30,22		S by W	1	Fine.
21	7	0	42	55	30,23		SW	1	Foggy.
	2	0	56	57	30,21		W	1	Fine.
22	7	0	42	55	30,11		SSW	1	Fine.
	2	0	56	56	30,02		SSW	1	Hazy.
23	7	0	46	55	29,48	0,019	S	1	Rainy.
	2	0	54	56	29,44		SSW	1	Fine.
24	7	0	38	54	29,51		NW	1	Fine.
	2	0	51	57	29,60		NNW	1	Fine.
25	7	0	42	55	29,75		SW	2	Fine.
	2	0	47	56	29,63		S	2	Rainy.
26	7	0	48	54	29.55	0,262	S by W	2	Cloudy.
	2	0	53	57	29,58		S by W	2	Cloudy.
27	7	0	44	54	29,70		SW	2	Fine.
	2	0	53	57	29,70		SW by W	2	Fine.
28	7	0	47	55	29,58		WSW	2	Cloudy.
	2	0	56	57	29,51		SW	2	Cloudy.
29	7	0	48	55	29,30	0,016	SW	1	Cloudy.
	2	0	53	56	29,28		W by S	1	Cloudy.
30	7	0	48	56	29,16	0,120	WSW	1	Cloudy.
	2	0	52	57	29,26		SW by S	1	Cloudy.
31	7	0	42	56	29,51	0,025	SSW	1	Fine.
	2	0	52	58	29,59		SSW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
April 1	7	0	45	56	29,98		ENE	1	Cloudy.
	2	0	49	57	30,02		NE	1	Cloudy.
2	7	0	44	54	29,96		NE	1	Cloudy.
	2	0	48	55	29,88		ENE	2	Cloudy.
3	7	0	45	54	29,68	0,117	ENE	1	Cloudy.
	2	0	49	55	29,66		E	1	Rainy.
4	7	0	44	54	29,74	0,049	NNE	1	Cloudy.
	2	0	53	57	29,78		ENE	1	Fine.
5	7	0	45	55	29,84		NE	1	Cloudy.
	2	0	52	56	29,82		NE	1	Cloudy.
6	7	0	48	55	29,76		SSE	1	Rainy.
	2	0	51	57	29,72		SE	1	Cloudy.
7	7	0	45	55	29,77	0,021	NE	2	Cloudy.
	2	0	49	57	29,85		E by N	2	Fine.
8	7	0	42	55	30,08		ENE	2	Cloudy.
	2	0	47	56	30,12		NE by E	2	Cloudy.
9	7	0	38	53	30,28		ENE	2	Fine.
	2	0	52	55	30,24		ENE	2	Fine.
10	7	0	40	53	30,24		ENE	1	Fine.
	2	0	56	57	30,16		NE by E	1	Fine.
11	7	0	43	55	30,07	0,027	SE by E	1	Cloudy.
	2	0	52	56	30,07		E	1	Hazy.
12	7	0	40	54	30,07		NE	1	Cloudy.
	2	0	54	57	30,02		NE by E	1	Fine.
13	7	0	45	55	29,92	0,034	N	1	Cloudy.
	2	0	47	56	29,88		NNW	2	Cloudy.
14	7	0	37	55	29,84		NNE	2	Hazy.
	2	0	48	55	29,78		NNE	2	Cloudy.
15	7	0	40	55	29,80		S	1	Fine.
	2	0	53	57	29,88		S	2	Cloudy.
16	7	0	45	54	30,09		NE	1	Cloudy.
	2	0	53	57	30,13		NE by E	2	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
April 17	7	0	40	54	30,40		NE	1	Cloudy.
	2	0	43	54	30,40		NE	1	Cloudy.
18	7	0	45	54	30,13		N	2	Cloudy.
	2	0	51	55	30,00		NW	2	Cloudy.
19	7	0	45	54	30,08	0,023	NNW	2	Fine.
	2	0	52	54	30,13		N	2	Cloudy.
20	7	0	42	53	30,34		NE	2	Cloudy.
	2	0	46	55	30,38		NE	2	Cloudy.
21	7	0	41	53	30,48		NE	2	Cloudy.
	2	0	51	55	30,42		NE	2	Cloudy.
22	7	0	44	53	30,27		NW	2	Cloudy.
	2	0	57	55	30,22		W by N	2	Cloudy.
23	7	0	48	53	30,20		W	2	Cloudy.
	2	0	58	56	30,18		W	2	Cloudy.
24	7	0	47	54	30,20		NE	1	Cloudy.
	2	0	56	57	30,22		NE	1	Cloudy.
25	7	0	48	55	29,88		W by S	1	Cloudy.
	2	0	50	56	29,56		SW	1	Rainy.
26	7	0	47	55	29,57	0,197	NNW	2	Fine.
	2	0	55	57	29,68		NW	2	Cloudy.
27	7	0	47	56	29,77		WSW	1	Fine.
	2	0	54	58	29,57		SW	1	Rainy.
28	7	0	46	56	29,07	0,170	WSW	2	Fair.
	2	0	49	57	29,17		W	2	Cloudy.
29	7	0	46	55	29,27	0,052	SW	2	Cloudy.
	2	0	49	57	29,19		W by N	2	Cloudy.
30	7	0	42	54	29,32	0,233	NW	2	Cloudy.
	2	0	45	55	29,48		N	2	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
May 1	7	0	39	53	29,82		NNE	2	Fine.
	2	0	50	55	29,87		N	2	Cloudy.
2	7	0	42	53	29,97		SSE	1	Cloudy.
	2	0	58	55	29,92		W by S	1	Fine.
3	7	0	50	55	29,93		W	1	Fine.
	2	0	60	57	29,88		WSW	1	Fine.
4	7	0	55	55	29,82		WSW	1	Cloudy.
	2	0	62	59	29,91			0	Cloudy.
5	7	0	46	57	30,08		NNW	2	Fine.
	2	0	54	59	30,21		NNE	2	Fine.
6	7	0	41	56	30,30		NNE	1	Fine.
	2	0	51	57	30,28		NE	2	Cloudy.
7	7	0	43	56	30,28		NE by E	1	Fine.
	2	0	57	59	30,28		NE by E	1	Fine.
8	7	0	48	57	30,16		NE	1	Fine.
	2	0	62	59	30,05		NE	1	Fine.
9	7	0	49	57	29,84		NNE	1	Fine.
	2	0	60	60	29,76		NE	1	Hazy.
10	7	0	46	58	29,66		NE	1	Cloudy.
	2	0	49	60	29,54		N	1	Rainy.
11	7	0	47	58	29,45	0,631	SW	1	Cloudy.
	2	0	55	59	29,55		SW	1	Cloudy.
12	7	0	44	58	29,76		W	1	Hazy.
	2	0	61	60	29,76		W	1	Cloudy.
13	7	0	50	59	29,74		WSW	1	Fine.
	2	0	64	60	29,70		W by S	1	Fine.
14	7	0	52	59	29,80		E by N	1	Fine.
	2	0	63	61	29,85		E by N	1	Cloudy.
15	7	0	50	60	30,07		E	1	Fine.
	2	0	56	60	30,09		ESE	1	Cloudy.
16	[illegible]	0	48	60	30,12		SE	2	Cloudy.
	[illegible]	0	54	60	30,11		SE	2	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Wind.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
May 17	7	0	49	60	30,02		SE	2	Fine.
	2	0	56	61	30,02		SE	2	Fine.
18	7	0	49	60	30,11		ESE	1	Fine.
	2	0	60	61	30,15		SE by E	1	Fine.
19	7	0	52	60	30,25		SSE	1	Fine.
	2	0	65	62	30,26		SSE	1	Fine.
20	7	0	52	61	30,26		SSE	1	Fine.
	2	0	66	62	30,26		SE	2	Fine.
21	7	0	57	62	30,18		SE	1	Cloudy.
	2	0	70	63	30,14		SE	1	Fine.
22	7	0	56	62	30,08		W	1	Fine.
	2	0	71	64	30,06		NW	1	Fine.
23	7	0	53	63	29,97		W	1	Cloudy.
	2	0	58	63	29,85		W	1	Cloudy.
24	7	0	49	61	29,56	0,137	W	2	Rainy.
	2	0	55	63	29,33		W	2	Fair.
25	7	0	50	62	29,50	0,100	WNW	1	Cloudy.
	2	0	61	63	29,63		WNW	1	Fine.
26	7	0	53	62	29,53	0,060	WNW	2	Fine.
	2	0	64	62	29,49		W	2	Cloudy.
27	7	0	52	61	29,56	0,130	W by N	1	Cloudy.
	2	0	62	62	29,58		W by N	1	Fine.
28	7	0	52	61	29,45	0,059	W	2	Cloudy.
	2	0	59	62	29,46		WNW	2	Cloudy.
29	7	0	49	60	29,77	0,020	WNW	2	Cloudy.
	2	0	57	62	29,83		WNW	2	Cloudy.
30	7	0	50	59	29,93		NNE	1	Fine.
	2	0	61	61	30,08		NNE	1	Fine.
31	7	0	52	60	30,31		NE	1	Fine.
	2	0	69	61	30,29		NNE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
June 1	7	0	57	60	30,21		SE	1	Fine.
	2	0	65	62	30,10		N	1	Cloudy.
2	7	0	51	61	30,00		NNE	1	Cloudy.
	2	0	55	61	30,01		NNE	1	Cloudy.
3	7	0	51	61	29,96		SSW	1	Fine.
	2	0	65	62	29,90		NW	1	Fine.
4	7	0	51	60	29,96		NE	1	Cloudy.
	2	0	62	61	29,91		NW	1	Cloudy.
5	7	0	52	61	29,67		SSW	1	Fine.
	2	0	64	62	29,64		WSW	1	Cloudy.
6	7	0	49	60	29,77		NE	1	Cloudy.
	2	0	50	61	29,81		NE by E	1	Cloudy.
7	7	0	46	59	30,00	0,076	SW	1	Fine.
	2	0	60	60	30,00		SW	1	Fine.
8	7	0	51	59	30 14		SSE	1	Fine.
	2	0	63	60	30,13		SE by S	1	Fine.
9	7	0	59	60	30,18		SE by S	1	Fine.
	2	0	69	62	30,16		SE by S	1	Fine.
10	7	0	58	60	30,14		SE by E	1	Fine.
	2	0	68	63	30,11		SE	1	Fine.
11	7	0	58	63	30,05		E by S	1	Fine.
	2	0	73	64	30,04		E by S	1	Fine.
12	7	0	58	63	30,04		E	1	Fine.
	2	0	77	66	30,00		E	1	Fine.
13	7	0	58	64	29,97		ENE	1	Cloudy.
	2	0	64	65	29,90		ENE	1	Cloudy.
14	7	0	55	65	29,70	0,175	E	1	Rainy.
	2	0	63	65	29,68		SSW	2	Cloudy.
15	7	0	58	65	29,68		SSE	1	Fine.
	2	0	73	65	29,68		SSE	1	Hazy.
16	7	0	59	65	29,90		S by E	1	Fine.
	2	0	73	67	29,94		S	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
June 17	7	0	58	66	30,04		SW	1	Fine.
	2	0	67	66	30,05		SW	1	Cloudy.
18	7	0	58	65	30,08		SW	1	Fine.
	2	0	66	65	30,00		SW	1	Cloudy.
19	7	0	55	65	29,83	0,065	WSW	1	Cloudy.
	2	0	64	65	29,81		SW by W	1	Cloudy.
20	7	0	54	63	29,80		WSW	1	Cloudy.
	2	0	67	65	29,75		SW	1	Cloudy.
21	7	0	55	64	29,76		SE	1	Hazy.
	2	0	62	64	29,74		SE	1	Cloudy.
22	7	0	58	64	29,70	0,020	SSW	1	Cloudy.
	2	0	68	65	29,74		SW	1	Fine.
23	7	0	60	65	29,79	0,017	SW by W	1	Cloudy.
	2	0	71	66	29,87		WSW	1	Cloudy.
24	7	0	58	65	30,01		SSE	1	Fine.
	2	0	71	67	29,94		S by W	1	Fine.
25	7	0	59	66	29,71	0,209	SW	2	Cloudy.
	2	0	67	66	29,72		SW	2	Cloudy.
26	7	0	57	65	29,78		SW by S	1	Cloudy.
	2	0	62	66	29,78		SSW	2	Cloudy.
27	7	0	58	66	29,55	0,100	SSW	2	Cloudy.
	2	0	67	66	29,55		S by W	2	Cloudy.
28	7	0	56	65	29,82		SW	1	Hazy.
	2	0	65	66	29,82		SW by S	1	Cloudy.
29	7	0	55	65	29,86		SSW	1	Fine.
	2	0	68	66	29,88		SSW	1	Hazy.
30	7	0	56	65	29,98		SW	1	Hazy.
	2	0	72	67	30,03		SW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 1	7	0	61	65	30,22		SE by E	1	Fine.
	2	0	73	67	30,26		ESE	1	Cloudy.
2	7	0	59	66	30,38		ESE	1	Fine.
	2	0	77	68	30,33		ESE	1	Fine.
3	7	0	63	67	30,38		N	1	Fine.
	2	0	78	69	30,35		NNE	1	Fine.
4	7	0	66	69	30,37		NE	1	Fine.
	2	0	77	71	30,37		NE	1	Fine.
5	7	0	65	70	30,24		ESE	1	Fine.
	2	0	78	72	30,18		ESE	1	Fine.
6	7	0	62	70	29,94		SE	1	Fine.
	2	0	77	73	29,80		S	1	Fine.
7	7	0	60	70	29,70		SW	1	Cloudy.
	2	0	71	70	29,70		W	1	Fine.
8	7	0	58	69	29.70		WSW	1	Fine.
	2	0	71	70	29,76		WSW	1	Fine.
9	7	0	59	68	29,95		WSW	1	Fine.
	2	0	65	67	29,94		WSW	1	Cloudy.
10	7	0	57	67	29,73	0,131	SSW	1	Cloudy.
	2	0	68	68	29,74		SW	1	Cloudy.
11	7	0	55	65	29,98	0,083	SW	1	Fine.
	2	0	67	69	29,97		SW	1	Fine.
12	7	0	58	65	29,73	0,190	WSW	1	Cloudy.
	2	0	67	66	29,69		WSW	1	Cloudy.
13	7	0	58	66	29,66	0,412	SW	1	Cloudy.
	2	0	58	65	29,61		SSW	1	Rainy.
14	7	0	57	65	29,55	0,230	NNE	1	Fair.
	2	0	65	65	29,59		NE	1	Cloudy.
15	7	0	56	65	29,75	0,045	SSE	1	Cloudy.
	2	0	66	66	29,71		S	1	Cloudy.
16	7	0	57	66	29,60	0,244	SSW	2	Fine.
	2	0	65	66	29,74		SSW	2	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 17	7	0	55	65	29,73	0,094	S by W	1	Rainy.
	2	0	63	65	29,73		SSW	2	Rainy.
18	7	0	54	64	29,94	0,183	SW by W	1	Fine.
	2	0	62	64	29,93		SW	1	Fine.
19	7	0	51	63	30,04	0,044	WSW	1	Fine.
	2	0	66	64	30,04		W	1	Cloudy.
20	7	0	58	63	29,88		SW by S	1	Cloudy.
	2	0	62	64	29,78		SW by S	1	Cloudy.
21	7	0	57	63	29,52	0,015	SW by S	1	Cloudy.
	2	0	60	64	29,47		SW by S	1	Rainy.
22	7	0	56	63	29,44	0,220	SW by W	1	Cloudy.
	2	0	63	64	29,44		SW by W	1	Fine.
23	7	0	58	63	29,49	0,173	SSW	1	Cloudy.
	2	0	63	64	29 49		SSW	1	Rainy.
24	7	0	56	64	29,37	0,305	SW by S	1	Cloudy.
	2	0	60	64	29,40		SW by S	1	Rainy.
25	7	0	57	63	29,54	0,195	SW by S	1	Cloudy.
	2	0	63	64	29,51		SW	1	Cloudy.
26	7	0	55	63	29,71	0,235	SW by W	1	Fine.
	2	0	64	64	29,75		SW by W	1	Fine.
27	7	0	56	63	29,92	0,111	WSW	1	Fine.
	2	0	66	64	29,94		SW	1	Fine.
28	7	0	57	64	30,00	0,161	S	1	Fine.
	2	0	67	65	29,95		S	1	Hazy.
29	7	0	62	65	29,64		SW	1	Cloudy.
	2	0	64	65	29,66		SW	1	Cloudy.
30	7	0	62	65	29,71	0,053	SW by S	2	Cloudy.
	2	0	67	66	29,70		SW	2	Fine.
31	7	0	54	65	29,80	0,122	SW by S	1	Rainy.
	2	0	66	66	29,77		SW by S	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Auguſt 1	7	0	57	65	29,94	0,425	SW	2	Cloudy.
	2	0	68	66	29,94		SW	2	Fine.
2	7	0	56	65	30,18		WSW	1	Fine.
	2	0	67	67	30,26		WSW	1	Fine.
3	7	0	57	67	30,40		WSW	1	Fine.
	2	0	75	68	30,40		W	1	Fine.
4	7	0	60	67	30,38		WSW	1	Fine.
	2	0	77	68	30,36		WSW	1	Fine.
5	7	0	64	68	30,18		ESE	1	Hazy.
	2	0	78	68	29,98		SE	1	Fine.
6	7	0	62	68	30,02		SW	1	Cloudy.
	2	0	75	69	30,04		WSW	1	Cloudy.
7	7	0	61	70	30,16		ESE	1	Fine.
	2	0	77	71	30,16		ESE	1	Fine.
8	7	0	63	70	30,22		SE	1	Fine.
	2	0	77	72	30,18		SE	1	Fine.
9	7	0	63	71	30,06		ESE	1	Fine.
	2	0	83	73	29,99		E by S	1	Fine.
10	7	0	64	72	29,91		WSW	1	Cloudy.
	2	0	77	73	30,00		WSW	1	Cloudy.
11	7	0	63	71	30,04	0,060	WSW	1	Cloudy.
	2	0	74	71	30,05		WSW	1	Fine.
12	7	0	58	70	30,86	0,036	SW	1	Cloudy.
	2	0	67	70	29,86		SW	1	Cloudy.
13	7	0	55	68	30,23	0,044	SW	1	Cloudy.
	2	0	72	69	30,22		SW	1	Fine.
14	7	0	55	68	30,13		WSW	1	Cloudy.
	2	0	67	68	30,03		WSW	2	Cloudy.
15	7	0	62	68	29,90	0,075	SW by W	2	Rainy.
	2	0	68	68	29,90		SW by W	1	Fair.
16	7	0	63	68	29,90		SW	1	Cloudy.
	2	0	67	68	29,87		SW	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain .	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Auguſt 17	7	0	57	68	29,70		SW by W	2	Cloudy.
	2	0	68	68	29,73		SW by W	2	Cloudy.
18	7	0	54	66	29,81	0,068	SW by S	2	Fine.
	2	0	65	66	29,76		SSW	2	Hazy.
19	7	0	54	66	29,72	0,060	SSW	1	Fine.
	2	0	67	66	29,70		SSW	1	Cloudy.
20	7	0	57	65	29,92		W	1	Cloudy.
	2	0	68	66	29,96		W	1	Fine.
21	7	0	54	66	30,18		WNW	1	Fine.
	2	0	69	66	30,21		W	1	Cloudy.
22	7	0	54	66	30,20		W	1	Fine.
	2	0	70	66	30,10		WSW	1	Fine.
23	7	0	59	67	29,89	0,059	WSW	1	Cloudy.
	2	0	70	67	29,89		WSW	1	Cloudy.
24	7	0	52	65	29,87		WSW	1	Cloudy.
	2	0	58	66	29,70		WSW	1	Cloudy.
25	7	0	52	65	29,28	0,107	WSW	1	Cloudy.
	2	0	54	66	29,25		WSW	2	Rainy.
26	7	0	48	62	29,68	0,220	WSW	2	Fine.
	2	0	63	63	29,72		WSW	2	Cloudy.
27	7	0	54	63	29,85	0,017	NNW	2	Cloudy.
	2	0	63	63	29,92		NNW	1	Cloudy.
28	7	0	54	63	30,05		NNW	1	Cloudy.
	2	0	63	63	30,05		NNW	1	Cloudy.
29	7	0	54	62	30,05		WNW	1	Cloudy.
	2	0	68	63	30,05		WNW	1	Cloudy.
30	7	0	58	63	30,15		WNW	1	Cloudy.
	2	0	65	63	30,23		WNW	1	Cloudy.
31	7	0	58	63	30,34		NW	1	Cloudy.
	2	0	67	64	30,35		NNW	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Sept. 1	7	0	58	63	30,38		NNE	1	Cloudy.
	2	0	62	63	30,38		NNE	1	Cloudy.
2	7	0	55	63	30,40		NE	1	Cloudy.
	2	0	63	63	30,40		NE	1	Cloudy.
3	7	0	54	62	30,44		ENE	1	Cloudy.
	2	0	61	63	30,45		ENE	1	Cloudy.
4	7	0	57	62	30,40		ENE	1	Cloudy.
	2	0	64	63	30,36		ENE	1	Cloudy.
5	7	0	60	62	30,29		ESE	1	Cloudy.
	2	0	68	63	30,24		ESE	2	Cloudy.
6	7	0	58	63	30,11		ESE	1	Cloudy.
	2	0	65	64	30,05		ESE	1	Cloudy.
7	7	0	57	63	30,02		SE	1	Cloudy.
	2	0	65	64	30,02		SE	1	Cloudy.
8	7	0	56	63	30,13		SE	1	Cloudy.
	2	0	66	64	30,03		SE	1	Cloudy.
9	7	0	56	63	30,18		SE	1	Cloudy.
	2	0	64	64	30,18		SE	1	Cloudy.
10	7	0	52	62	30,23		SSE	1	Fine.
	2	0	62	63	30,22		SE	1	Cloudy.
11	7	0	53	62	30,28		SSE	1	Cloudy.
	2	0	63	63	30,28		SE	1	Cloudy.
12	7	0	53	61	30,30		ESE	1	Cloudy.
	2	0	61	62	30,28		E	1	Cloudy.
13	7	0	53	61	30,23		ESE	1	Cloudy.
	2	0	61	62	30,18		SE	1	Cloudy.
14	7	0	53	61	30,10		NE	1	Cloudy.
	2	0	62	61	30,05		ENE	1	Cloudy.
15	7	0	54	61	29,93		SSE	1	Cloudy.
	2	0	62	62	29,86		SSE	1	Cloudy.
16	7	0	57	61	29,46	0,160	SSE	1	Rainy.
	2	0	64	62	29,30		SSW	1	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Sept. 17	7	0	52	61	29,26	0,151	SW	1	Fine.
	2	0	58.	62	29,11		SW	1	Rainy.
18	7	0	53	62	29,18	0,135	WSW	1	Cloudy.
	2	0	62	62	29,29		WSW	1	Fine.
19	7	0	51	61	29,47	0,162	WSW	1	Fine.
	2	0	58	62	29,45		WSW	1	Rainy.
20	7	0	47	60	29,78	0,066	WSW	1	Fine.
	2	0	63	62	29,78		WSW	2	Cloudy.
21	7	0	58	61	29,51	0,155	SSE	2	Cloudy.
	2	0	63	62	29,54		SSE	2	Cloudy.
22	7	0	56	62	29,68	0,046	SE by S	2	Rainy.
	2	0	65	62	29,61		S	2	Fair.
23	7	0	55	62	29,85	0,017	SSW	2	Cloudy.
	2	0	65	63	29,86		SSW	2	Fine.
24	7	0	57	62	29,92		SSW	1	Cloudy.
	2	0	65	63	29.94		SSW	2	Cloudy.
25	7	0	53	63	29,88	0,098	SSE	1	Foggy.
	2	0	65	64	29,77		SE	1	Cloudy.
26	7	0	59	63	29,68		ESE	1	Cloudy.
	2	0	68	64	29,70		SSE	2	Fine.
27	7	0	56	64	29,76		S	1	Foggy.
	2	0	57	63	29,80		S	1	Cloudy.
28	7	0	48	62	29,98		S	1	Cloudy.
	2	0	57	62	30,00		S	1	Cloudy.
29	7	0	46	61	30,00		S	1	Fine.
	2	0	59	61	29,95		SE	1	Cloudy.
30	7	0	49	61	29,83		ESE	1	Cloudy.
	2	0	59	61	29,72		SE	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Oct. 1	7	0	54	61	29,56		SE	1	Cloudy.
	2	0	63	62	29,55		SE	1	Cloudy.
2	7	0	58	61	29,71		E	1	Cloudy.
	2	0	63	62	29,75		ESE	1	Fine.
3	7	0	56	61	29,65		S	1	Cloudy.
	2	0	57	61	29,60		S	1	Small rain.
4	7	0	56	61	29,56		SSW	2	Cloudy.
	2	0	57	61	29,56		SSW	2	Cloudy.
5	7	0	51	61	29,78	0,125	SSW	2	Fine.
	2	0	58	60	29,82		SSW	2	Cloudy.
6	7	0	54	59	29,96	0,076	SSE	1	Cloudy.
	2	0	62	60	29,99		SSE	1	Cloudy.
7	7	0	54	60	30,04	0,212	ESE	1	Cloudy.
	2	0	62	61	30,08		ESE	1	Cloudy.
8	7	0	56	60	30,10		SE	1	Cloudy.
	2	0	61	61	30,09		SE	1	Cloudy.
9	7	0	47	60	30,09		SSE	1	Foggy.
	2	0	62	61	30,03		S	1	Cloudy.
10	7	0	56	60	29,53		SSW	1	Cloudy.
	2	0	56	60	29,27		SSW	2	Cloudy.
11	7	0	44	58	29,28	0,170	SSW	1	Cloudy.
	2	0	50	58	29,25		SSW	1	Cloudy.
12	7	0	39	56	29,36		SSW	2	Cloudy.
	2	0	51	57	29,35		SSW	2	Fine.
13	7	0	41	56	29,21		SW by S.	1	Fine.
	2	0	56	57	29,24		SW	1	Fine.
14	7	0	41	56	29,42		SW by S.	1	Foggy.
	2	0	55	57	29,57		SW by S	1	Hazy.
15	7	0	39	55	29,78		SW	1	Fine.
	2	0	56	57	29,77		SW	1	Fine.
16	7	0	55	56	29,55	0,315	WSW	1	Cloudy.
	2	0	60	58	29,66		WSW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Oct. 17	7	0	55	57	29,74	0,015	SW by W	1	Cloudy.
	2	0	56	56	29,72		SW by W	1	Cloudy.
18	7	0	46	56	29,64	0,105	SW by W	1	Foggy.
	2	0	55	57	29,67		SW by W	1	Fine.
19	7	0	42	56	29,72		SW by W	1	Fine.
	2	0	52	57	29,78		WSW	1	Fine.
20	7	0	45	56	29,98		WSW	1	Fine.
	2	0	53	57	30,05		WSW	1	Cloudy.
21	7	0	45	55	30,15		WNW	1	Cloudy.
	2	0	48	57	30,13		NNW	1	Cloudy.
22	7	0	48	54	29,67	0,038	SSW	2	Rainy.
	2	0	56	56	29,57		SSW	2	Fair.
23	7	0	44	55	29,68		SW	1	Fine.
	2	0	54	57	29,77		WSW	1	Cloudy.
24	7	0	41	55	29,90		SW	1	Fine.
	2	0	52	56	29,72		SSW	1	Rainy.
25	7	0	44	55	29,79	0,223	SSW	1	Cloudy.
	2	0	53	57	29,47		S	2	Rainy.
26	7	0	47	56	29,60	0,284	WSW	1	Cloudy.
	2	0	52	57	29,70		WSW	1	Cloudy.
27	7	0	57	56	29,61	0,090	SW by S	1	Cloudy.
	2	0	59	60	29,72		SW by S	1	Cloudy.
28	7	0	58	58	29,90	0,130	S by W	1	Cloudy.
	2	0	60	58	29,94		SSW	1	Cloudy.
29	7	0	53	58	30,03		SW	1	Cloudy.
	2	0	58	60	30,07		SW	1	Rainy.
30	7	0	56	59	29,87	0,035	SW by S	1	Cloudy.
	2	0	60	61	29,78		SW	1	Cloudy.
31	7	0	53	60	29,55		SSW	1	Fine.
	2	0	58	62	29,42		SSW	2	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Nov. 1	7	0	49	61	29,39	0,020	SW	1	Hazy.
	2	0	56	61	29,38		SW	1	Fine.
2	7	0	45	59	29,38	0,195	SSE	1	Foggy.
	2	0	50	61	29,27		SSE	1	Cloudy.
3	7	0	40	58	29,55	0,268	NNE	2	Fine.
	2	0	48	59	29,83		NNE	2	Fine.
4	7	0	36	56	29,96		SSE	1	Fine.
	2	0	48	56	29,67		S	2	Cloudy.
5	7	0	42	55	29,58	0,135	SSE	2	Cloudy.
	2	0	48	57	29,60		SSW	2	Fine.
6	7	0	48	57	29,55		WSW	2	Cloudy.
	2	0	52	59	29,55		WSW	2	Fine.
7	7	0	43	56	29,61		WSW	1	Fine.
	2	0	49	58	29,70		WSW	1	Fine.
8	7	0	47	56	29,54		SSE	1	Cloudy.
	2	0	52	58	29,18		S	2	Rainy.
9	7	0	47	56	29,12	0,305	SW by W	2	Fine.
	2	0	53	58	29,48		W	2	Fine.
10	7	0	48	58	29,74		S by E	1	Fine.
	2	0	54	60	29,71		S	1	Cloudy.
11	7	0	51	58	29,45		SSE	1	Cloudy.
	2	0	58	60	29,33		SE by S	1	Fine.
12	7	0	49	57	29,42	0,080	S	2	Rainy.
	2	0	53	58	29,48		SSE	1	Cloudy.
13	7	0	43	58	29,48		ESE	1	Foggy.
	2	0	54	59	29,60		E	2	Cloudy.
14	7	0	42	57	29,80		N	1	Fine.
	2	0	43	58	29,80		NNW	1	Fine.
15	7	0	38	56	29,97		N	1	Cloudy.
	2	0	43	57	30,01		N	1	Fine.
16	7	0	36	55	30,10		NNE	1	Fine.
	2	0	43	57	30,16		NNW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Nov. 17	7	0	41	55	30,10		NNW	1	Cloudy.
	2	0	44	56	29,98		NNW	1	Cloudy.
18	7	0	36	54	29,84		NW	1	Cloudy.
	2	0	40	53	29,85		NW	1	Fine.
19	7	0	33	51	29,86		NWbyW	1	Cloudy.
	2	0	40	53	29,62		W	1	Rainy.
20	7	0	34	51	29,68	0,337	NNW	1	Fine.
	2	0	42	53	29,75		NNW	1	Fine.
21	7	0	32	50	30,00		N	1	Hazy.
	2	0	40	52	30,00		NW	1	Cloudy.
22	7	0	42	51	29,88		W	1	Cloudy.
	2	0	44	54	29,74		WSW	1	Cloudy.
23	7	0	35	52	29,71		W	1	Fine.
	2	0	42	54	29,73		WSW	1	Cloudy.
24	7	0	33	52	29,82		WSW	1	Fine.
	2	0	39	53	29,90		WSW	1	Fine.
25	7	0	29	51	30,12		WSW	1	Fine.
	2	0	33	52	30,16		WSW	1	Fine.
26	7	0	33	50	30,26		E	1	Fine.
	2	0	39	52	30,28		ESE	1	Fine.
27	7	0	32	50	30,28		ESE	1	Fine.
	2	0	36	51	30,28		ESE	1	Fine.
28	7	0	30	48	30,28		ESE	1	Fine.
	2	0	36	52	30,30		E	1	Fine.
29	7	0	31	49	30,45		E	1	Foggy.
	2	0	36	51	30,44		E	1	Fine.
30	7	0	29	48	30,38		E	1	Foggy.
	2	0	36	51	30,33		ESE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Dec. 1	8	0	34	48	30,17		SE	1	Fine.
	2	0	42	51	30,12		SE	1	Cloudy.
2	8	0	39	50	29,77		SE	1	Cloudy.
	2	0	43	52	29,64		SSE	1	Rainy.
3	8	0	39	50	29,57	0,025	S by W	1	Fine.
	2	0	46	52	29,60		S by W	1	Fine.
4	8	0	39	52	29,57		SW	1	Fine.
	2	0	48	54	29,61		SW	1	Cloudy.
5	8	0	48	53	29,39	0,845	SSW	1	Rainy.
	2	0	48	56	29,40		SW	1	Fair.
6	8	0	38	53	29,56	0,300	NNW	1	Cloudy.
	2	0	46	55	29,67		NNW	1	Cloudy.
7	8	0	37	53	29,88		NNW	1	Fine.
	2	0	47	55	29,94		E	1	Cloudy.
8	8	0	45	54	29,94	0,130	SSE	1	Cloudy.
	2	0	45	56	29,94		S	1	Cloudy.
9	8	0	53	55	29,71	0,440	SSW	2	Cloudy.
	2	0	53	56	29,75		SW	2	Fine.
10	8	0	48	55	29,87		SSW	1	Fine.
	2	0	50	56	29,87		SSW	1	Fine.
11	8	0	50	56	29,77		SSW	1	Cloudy.
	2	0	52	58	29,73		S by W	1	Fine.
12	8	0	48	56	29,47		S	1	Cloudy.
	2	0	53	58	29,53		S	1	Cloudy.
13	8	0	44	57	29,79		WSW	1	Cloudy.
	2	0	52	58	29,75		W	1	Fine.
14	8	0	48	57	29,45	0,195	S	2	Rainy.
	2	0	51	57	29,37		S	1	Fine.
15	8	0	47	58	29,38	0,040	W	1	Foggy.
	2	0	48	59	29,39		SW	1	Cloudy.
16	8	0	46	58	29,45	0,345	W	1	Cloudy.
	2	0	48	59	29,56		W	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Wind.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Dec. 17	8	0	42	57	29,53	0,038	E	1	Cloudy.
	2	0	45	57	29,43		E	1	Cloudy.
18	8	0	47	58	29,33	0,073	S	1	Rainy.
	2	0	50	57	29,30		S	1	Cloudy.
19	8	0	46	57	29,47	0,060	S	1	Cloudy.
	2	0	45	57	29,46		S	1	Cloudy.
20	8	0	45	55	29,38	0,100	SSW	1	Foggy.
	2	0	47	58	29,38		WSW	1	Foggy.
21	8	0	38	55	29,76		NE	1	Cloudy.
	2	0	40	55	29,78		NE	1	Cloudy.
22	8	0	35	54	29,70		ENE	1	Cloudy.
	2	0	38	55	29,65		E	1	Cloudy.
23	8	0	31	52	29,55		NE	1	Fine.
	2	0	35	54	29,49		NE	1	Fine.
24	8	0	30	50	29,35		ENE	1	Snow.
	2	0	32	53	29,30		NE	2	Snow.
25	8	0	32	50	29,21		N	2	Snow.
	2	0	33	51	29,33		N	2	Snow.
26	8	0	31	49	29,56		W	1	Cloudy.
	2	0	35	51	29,64		WSW	1	Cloudy.
27	8	0	35	49	29,96		NW	1	Cloudy.
	2	0	37	52	30,13		NNW	1	Fine.
28	8	0	32	48	30,40		N	1	Cloudy.
	2	0	36	52	30,40		N	1	Cloudy.
29	8	0	34	49	30,24		ESE	1	Fine.
	2	0	37	51	30,15		E	1	Cloudy.
30	8	0	37	48	29,91	0,409	E	1	Foggy.
	2	0	37	48	29,90		E	1	Foggy.
31	8	0	37	46	30,04		ESE	1	Foggy.
	2	0	39	50	30,06		ESE	1	Foggy.

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1787	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greateſt height.	Leaſt height.	Mean height.	Greateſt height.	Leaſt height.	Mean height.	Greateſt height.	Leaſt height.	Mean height.	Inches.
							Inches.	Inches.	Inches.	
January	51°	27°	39°	54°	45°	49½°	30,64	29,52	30,08	0,360
February	53	31	42	56	50	53	30,36	28,67	29,51	1,041
March	56	34	45	58	51½	54¼	30,51	29,03	29,77	1,283
April	58	37	47½	58	53	55½	30,48	29,07	29,77	0,923
May	71	39	55	64	53	58½	30,31	29,33	29,82	1,137
June	77	46	61½	67	59	63	30,29	29,55	29,92	0,662
July	78	51	64½	71	63	67	30,38	29,37	29,87	3,246
Auguſt	83½	48	65¼	73	62	67½	30,40	29,25	29,82	1,171
September	68	46	57	64	61	62½	30,45	29,11	29,78	0,990
October	63	39	51	62	54	58	30,15	29,21	29,68	1,818
November	56	29	42½	61	48	54½	30,45	29,12	29,78	1,340
December	53	30	41½	59	46	52½	30,40	29,21	29,80	3,000
Whole year			51			58			29,80	16,971

EXPLA-

EXPLANATION OF THE INSTRUMENTS.

The inſtruments with which the foregoing obſervations were made are the ſame that were uſed in former obſervations of this kind, a full account of which was given by Henry Cavendish, Eſq. F. R. S. in the LXVIth Volume of the Philoſophical Tranſactions; but as they have been moved from the ſituations they had at that time, it may not be amiſs to mention how they are placed now, in order the better to ſhew what degree of accuracy may be expected from them. There being no one ſituation for a thermometer out of doors ſo good as could be wiſhed, it became neceſſary to make uſe of two thermometers; each is placed out of a three-pair-of-ſtairs window, one facing E.N.E. and the other W.S.W. and they ſtand about two or three inches from the wall, that they may be the more expoſed to the air, and the leſs affected by the heat and cold of the houſe. As the ſun ſhines on the eaſtern part of the building in the morning, the thermometer to the weſtward is made uſe of for the morning obſervation during that ſeaſon of the year when the ſun riſes high enough to affect the other; for all other obſervations, that to the eaſtward is employed. Neither the building oppoſite, nor that on the ſouth ſide of the thermometer to the eaſt, are elevated above it in an angle of more than 13°; but the oppoſite building is not more than twenty-five feet diſtant. The thermometer to the weſtward will not be affected by any other building than one to the northward, which is elevated above it in an angle of 20°, and which is only twenty feet diſtant.

The thermometer within doors is placed cloſe to the barometer, the heights of which it is intended chiefly to correct; the windows of the room in which they are kept look to the weſtward, and in winter the room has conſtantly a fire in it.

The veſſel which receives the rain is fixed to a chimney at the top of the houſe, and riſes ſix inches above the chimney; it is not ſcreened from the rain by any building or chimnies, there being none higher than that to which it is fixed.

END OF PART I. OF VOL. LXXVIII.